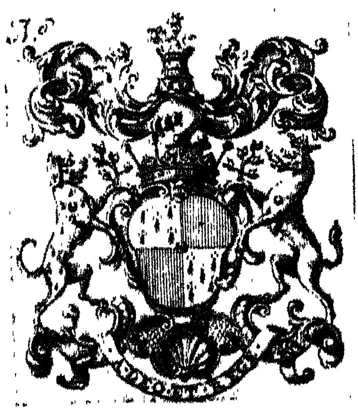

Agricultural Research Institute

PUSA

PHILOSOPHICAL

TRANSACTIONS,

OF THE

ROYAL SOCIETY

OF

LONDON.

FOR THE YEAR MDCCCXIII.

PART I.

LONDON,

PRINTED BY W. BULMER AND CO. CLEVELAND-ROW, ST. JAMES'S;
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MDCCCXIII.

ADVERTISEMENT.

THE Committee appointed by the *Royal Society* to direct the publication of the *Philosophical Transactions*, take this opportunity to acquaint the Public, that it fully appears, as well from the council-books and journals of the Society, as from repeated declarations which have been made in several former *Transactions*, that the printing of them was always, from time to time, the single act of the respective Secretaries, till the Forty-seventh Volume: the Society, as a Body, never interesting themselves any further in their publication, than by occasionally recommending the revival of them to some of their Secretaries, when, from the particular circumstances of their affairs, the *Transactions* had happened for any length of time to be intermitted. And this seems principally to have been done with a view to satisfy the Public, that their usual meetings were then continued, for the improvement of knowledge, and benefit of mankind, the great ends of their first institution by the Royal Charters, and which they have ever since steadily pursued.

But the Society being of late years greatly enlarged, and their communications more numerous, it was thought advisable that a Committee of their members should be appointed, to reconsider the papers read before them, and select out of them such as they should judge most proper for publication in the future *Transactions*; which was accordingly done upon the 26th of March, 1752. And the grounds of their choice are, and will continue to

be, the importance and singularity of the subjects, or the advantageous manner of treating them; without pretending to answer for the certainty of the facts, or propriety of the reasonings, contained in the several papers so published, which must still rest on the credit or judgment of their respective authors.

It is likewise necessary on this occasion to remark, that it is an established rule of the Society, to which they will always adhere, never to give their opinion, as a Body, upon any subject, either of Nature or Art, that comes before them. And therefore the thanks which are frequently proposed from the Chair, to be given to the authors of such papers as are read at their accustomed meetings, or to the persons through whose hands they receive them, are to be considered in no other light than as a matter of civility, in return for the respect shewn to the Society by those communications. The like also is to be said with regard to the several projects, inventions, and curiosities of various kinds, which are often exhibited to the Society; the authors whereof, or those who exhibit them, frequently take the liberty to report, and even to certify in the public newspapers, that they have met with the highest applause and approbation. And therefore it is hoped, that no regard will hereafter be paid to such reports and public notices; which in some instances have been too lightly credited, to the dishonour of the Society.

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Meteorological Journal kept at the Apartments of the Royal Society, by Order of the President and Council.

ERRATUM.

Page 50, line 4, *for* concave *read* convex.

PHILOSOPHICAL

TRANSACTIONS.

I. *On a new detonating Compound, in a Letter from Sir* Humphry Davy, *LL.D. F.R.S. to the Right Honourable Sir* Joseph Banks, *Bart. K.B. P.R.S.*

Read November 5, 1812.

My Dear Sir,

I think it right to communicate to you, and through you to the Royal Society, such circumstances as have come to my knowledge respecting a new and a very extraordinary detonating compound. I am anxious that those circumstances should be made public as speedily as possible, because experiments upon the substance may be connected with very dangerous results; and because I have already mentioned the mode of preparing it to many of my chemical friends, to whom my experience may be useful in saving them from danger.

About the end of September, I received a letter from a philosophical gentleman at Paris on some subjects of science, which contained the following paragraph:

"Vous avez sans doute appris, Monsieur, la découverte qu'on a faite à Paris il y a près d'un an, d'une combinaison de gaz azote et de chlorine, qui a l'apparence d'une huile plus

pesante que l'eau, et qui détonne avec toute la violence des métaux fulminans à la simple chaleur de la main, ce qui a privé d'un œil et d'un doigt l'auteur de cette découverte. Cette détonnation a lieu par la simple separation des deux gaz, comme celle de la combinaison d'oxigène et de chlorine ; il y a également beaucoup de lumière et de la chaleur produites dans cette détonnation, où un liquide se decompose en deux gaz."

The letter contained no account of the mode of preparation of this substance, nor any other details respecting it.

So curious and important a result could not fail to interest me, particularly as I have long been engaged in experiments on the action of azote and chlorine, without gaining any decided proofs of their power of combining with each other. I perused with avidity the different French chemical and physical journals, especially *Les Annales de Chimie*, and *Le Journal de Physique*, of which the complete series of last year have arrived in this country, in hopes of discovering some detail respecting the preparation of this substance, but in vain. I was unable to find any thing relative to it in these publications, or in the *Moniteur*.

It was evident from the notice, that it could not be formed in any operations in which heat is concerned; I therefore thought of attempting to combine azote and chlorine under circumstances which I had never tried before, that of presenting them to each other artificially cooled, the azote being in a nascent state. For this purpose I made a solution of ammonia, cooled it by a mixture of ice and muriate of lime, and slowly passed into it chlorine, cooled by the same means. There was immediately a violent action, accompanied by fumes of a pecu-

liarly disagreeable smell; at the same time a yellow substance was seen to form in minute films on the surface of the liquor; but it was evanescent, and immediately resolved itself into gas. I was preparing to repeat the experiment, substituting the prussiate of ammonia and other ammoniacal compounds, in which less heat might be produced by the action of the chlorine, than in the pure solution of the gas, when my friend, Mr. J. G. Children, put me in mind of a circumstance of which he had written to me an account, in the end of July, which promised to elucidate the enquiry, viz. that Mr. James Burton, jun. in exposing chlorine to a solution of nitrate of ammonia, had observed the formation of a yellow oil, which he had not been able to collect so as to examine its properties, as it was rapidly dissipated by exposure to the atmosphere. Mr. Children had tried the same experiment with similar results.

I immediately exposed a phial, containing about six cubical inches of chlorine, to a saturated solution of nitrate of ammonia, at the temperature of about 50° in common day-light. A diminution of the gas speedily took place; in a few minutes a film, which had the appearance of oil, was seen on the surface of the fluid; by shaking the phial it collected in small globules, and fell to the bottom. I took out one of the globules, and exposed it in contact with water to a gentle heat: long before the water began to boil, it exploded with a very brilliant light, but without any violence of sound.

I immediately proposed to Mr. Children, that we should institute a series of experiments upon its preparation and its properties. We consequently commenced the operations, the results of which I shall describe. We were assisted in our

labours, which were carried on in Mr. CHILDREN'S laboratory at Tunbridge, by Mr. WARBURTON.

It was found that the solution of oxalate of ammonia, or a very weak solution of pure ammonia, answered the purpose as well as the solution of nitrate of ammonia. It was formed most rapidly in the solution of ammonia, but it was white and clouded; and though less evanescent than in the strong solution I first used, it was far from being as permanent as in the solutions of nitrate and oxalate. The solution of prussiate of ammonia acted on by chlorine, afforded none of the peculiar oil; but produced white fumes, and became of a bright green colour. An attempt was made to procure the substance in large quantities, by passing chlorine into WOLFE'S bottles, containing the different solutions, but a single trial proved the danger of this mode of operating; the compound had scarcely began to form, when, by the action of some ammoniacal vapour on chlorine, heat was produced, which occasioned a violent explosion, and the whole apparatus was destroyed.

I shall now describe the properties of the new substance. Its colour is very nearly that of olive oil, and it is as transparent, and more perfectly liquid. I have not been able to ascertain its specific gravity with accuracy, but it is probably above 1.6. Its smell is very nauseous, strongly resembling that of the combination of carbonic oxide and chlorine, discovered by my brother; and its effect on the eyes is peculiarly pungent and distressing. A little of it was introduced under water into the receiver of an air pump, and the receiver exhausted; it became an elastic fluid, and in its gaseous state was rapidly absorbed or decomposed by the water. When warm water

was poured into a glass containing it, it expanded into a globule of elastic fluid, of an orange colour, which diminished as it passed through the water.

I attempted to collect the products of the explosion of the new substance, by applying the heat of a spirit lamp to a globule of it, confined in a curved glass tube over water: a little gas was at first extricated, but long before the water had attained the temperature of ebullition, a violent flash of light was perceived, with a sharp report; the tube and glass were broken into small fragments, and I received a severe wound in the transparent cornea of the eye, which has produced a considerable inflammation of the eye, and obliges me to make this communication by an amanuensis. This experiment proves what *extreme* caution is necessary in operating on this substance, for the quantity I used was scarcely as large as a grain of mustard seed.

A small globule of it thrown into a glass of olive oil, produced a most violent explosion; and the glass, though strong, was broken into fragments. Similar effects were produced by its action on oil of turpentine and naphtha. When it was thrown into ether there was a very slight action; gas was disengaged in small quantities, and a substance like wax was formed, which had lost the characteristic properties of the new body. On alcohol it acted slowly, lost its colour, and became a white oily substance, without explosive powers. When a particle of it was touched under water by a particle of phosphorus, a brilliant light was perceived under the water, and permanent gas was disengaged, having the characters of azote.

When quantities larger than a grain of mustard seed were used for the contact with phosphorus, the explosion was always

so violent as to break the vessel in which the experiment was made. The new body, when acted upon under water by mercury, afforded a substance having the appearance of corrosive sublimate, and gas was disengaged. On tin foil and zinc it exerted no action; it had no action on sulphur, nor on resin. In their alcoholic solutions it disappeared as in pure alcohol. It detonated most violently when thrown into a solution of phosphorus in ether, or in alcohol. Phosphorus introduced into ether, into which a globule of the substance had been put immediately before, produced no effect. In muriatic acid it gave off gas rapidly, and disappeared without explosion. On dilute sulphuric acid it exerted no violent action. It immediately disappeared without explosion in LIBAVIUS'S liquor, to which it imparted a yellow tinge.

It seems probable, from the general tenor of these facts, that the new substance is a compound of azote and chlorine; the same as, or analogous to, that mentioned in the letter from Paris. It is easy to explain its production in our experiments: the hydrogen of the ammonia may be conceived to combine with one portion of the chlorine to form muriatic acid, and the azote to unite with another portion of chlorine to form the new compound. The heat and light produced during its expansion into gaseous matter, supposing it to be composed of azote and chlorine, is without any parallel instance, in our present collection of chemical facts; the decomposition of euchlorine, which has been compared to it, is merely an expansion of matter already gaseous. The heat and light produced by its rarefaction, in consequence of decomposition, depend, probably, on the same cause as that which produces the flash of light in the discharge of the air gun.

The mechanical force of this compound in detonation, seems to be superior to that of any other known, not even excepting the ammoniacal fulminating silver. The velocity of its action appears to be likewise greater.

I am, my dear Sir,

with great respect, very sincerely your's,

H. DAVY.

II. *On a remarkable Application of* Cotes's *Theorem. By* J. F. W. Herschel, *Esq. Communicated by* W. Herschel, *LL. D. F. R. S.*

Read November 12, 1812.

Let a represent the semi-transverse axis of a conic section, ae the eccentricity, and consequently $a(1-e^2) = p$ the semi-parameter.

Let also $\lambda = \frac{e}{1+\sqrt{1-e^2}}$ and $\lambda' = \frac{e^{-1}}{1+\sqrt{1-e^{-2}}}$.

$r^{(1)}$ = the distance between a point in the curve, and the focus, which, for distinction's sake, we shall call the first focus, and the adjacent vertex the first vertex: the others the second.

$r^{(2)}$ = the distance between the same point and the second focus.

R = its distance from the centre.

ρ = its distance from the first vertex.

θ = the angle contained between the $r^{(1)}$, and the *prolongation* of a line joining the first vertex and focus.

φ = the angle contained between the R and a line joining the first vertex and centre.

ψ = the angle contained between the ρ and the same line.

$$\tan.\tfrac{1}{2}\varpi = \sqrt{\frac{1-e}{1+e}}.\cot.\tfrac{1}{2}\theta = \frac{1-\lambda}{1+\lambda}.\cot.\tfrac{1}{2}\theta = \sqrt{-1}.\frac{1-\lambda'}{1+\lambda'}.\cot.\tfrac{1}{2}\theta.$$

θ is the angle whose supplement is, in physical astronomy, known by the name of "true anomaly," and ϖ is the corresponding "eccentric anomaly."

The following equations are readily obtained.

$$r^{(1)} = \frac{a(1-e^2)}{1-e\,.\cos.\theta}; \text{ and } r^{(2)} = 2a - r^{(1)} = a\,.\frac{1-2e\,.\cos.\theta+e^2}{1-e\,.\cos.\theta}.$$

$$r^{(1)} = a\,(1-e\,.\cos.\varpi)\,;\; r^{(2)} = a\,(1+e\,.\cos.\varpi)$$

$$R^2 = \frac{a^2(1-e^2)}{1-e^2.(\cos.\varphi)^2}$$

$$\rho = \frac{2a(1-e^2)\,.\cos.\psi}{1-e^2\,.(\cos.\psi)^2} = \frac{2p\,.\cos.\psi}{1-e^2\,.(\cos.\psi)^2}.$$

$$2e = \lambda' + \lambda'^{-1} \text{ and } 2\,.e^{-1} = \lambda + \lambda^{-1}.$$

Hence we deduce the following

$$r^{(1)} = a\,.\frac{(1-e^2)(1+\lambda^2)}{1-2\lambda\,.\cos.\theta+\lambda^2} \quad \ldots\ldots \quad \{1\}$$

$$r^{(2)} = a\,(1+\lambda^2)\,.\frac{1-2e\,.\cos.\theta+e^2}{1-2\lambda\,.\cos.\theta+\lambda^2} \quad \ldots\ldots \quad \{2\}$$

$$\frac{r^{(2)}}{r^{(1)}} = \frac{1-2e\,.\cos.\theta+e^2}{1-e^2} \quad \ldots\ldots \quad \{3\}$$

again $r^{(1)} = a\,.\frac{1-2\lambda\,.\cos.\varpi+\lambda^2}{1+\lambda^2}$ $\quad \ldots\ldots \quad \{4\}$

$$r^{(2)} = a\,.\frac{1-2\lambda\,.\cos.(\pi-\varpi)+\lambda^2}{1+\lambda^2} \quad \ldots\ldots \quad \{5\}$$

where $\pi = 4\left\{1 - \frac{1}{3} + \frac{1}{5} - \frac{1}{7} + \&c.\right\}$.

$$\frac{r^{(1)}}{r^{(2)}} = \frac{1-2\lambda\,.\cos.\varpi+\lambda^2}{1-2\lambda\,.\cos.(\pi-\varpi)+\lambda^2} \quad \ldots\ldots \quad \{6\}$$

$$R^2 = \frac{a^2(1-e^2)(1+\lambda^2)^2}{\left\{1-2\lambda\,.\cos.\varphi+\lambda^2\right\}\left\{1-2\lambda\,.\cos.(\pi-\varphi)+\lambda^2\right\}} \quad \ldots\ldots \quad \{7\}$$

$$\rho = \frac{2a(1-e^2)(1+\lambda^2)^2\,.\cos.\psi}{\left\{1-2\lambda\,.\cos.\psi+\lambda^2\right\}\left\{1-2\lambda\,.\cos.(\pi-\psi)+\lambda^2\right\}} \quad \ldots\ldots \quad \{8\}$$

And lastly, since $1-e\,.\cos.\varpi = \frac{1-e^2}{1-e\,.\cos.\theta}$, we find $\cos.\theta =$

$$e^{-1} + \frac{e-e^{-1}}{1-e\,.\cos.\varpi} = \frac{e(1-e^{-1}\,.\cos.\varpi)}{1-e\,.\cos.\varpi}$$

$$\left.\begin{aligned} &= e\,.\frac{1+\lambda^2}{1+\lambda'^2}\,.\frac{1-2\lambda'\,.\cos.\varpi+\lambda'^2}{1-2\lambda\,.\cos.\varpi+\lambda^2} \\ &= e^{-1}\,.\left\{\frac{\lambda}{\lambda'}\right\}\,.\frac{1-2\lambda'\,.\cos\,\varpi+\lambda'^2}{1-2\lambda\,.\cos.\varpi+\lambda^2}.\end{aligned}\right\} \quad \ldots \quad \{9\}$$

Before we proceed to the application of these transformations, it will be necessary to premise some properties of the functions λ and λ'.

Let $\alpha = \cos.^{-1}(e^{-1})$ and $\alpha' = \cos.^{-1} e.$*

* This notation $\cos.^{-1} e$ must not be understood to signify $\frac{1}{\cos. e}$, but what is usually written thus, arc $(\cos. = e)$. It is true that many authors use $\cos.^{m} A$, $\text{Sin.}^{m} A$, &c. for $(\cos. A)^{m}$, $\text{Sin.} A^{m}$; lest therefore the notation here adopted should appear capricious, it will not be irrelevant to explain its grounds. If φ be the characteristic mark of an operation performed on any symbol, x, $\varphi(x)$ may represent the result of that operation. Now to denote the repetition of the same operation, instead of $\varphi(\varphi(x))$; $\varphi(\varphi(\varphi(x)))$; &c. we may most elegantly write $\varphi^2(x)$; $\varphi^3(x)$; &c. Thus we use d^2x, $\Delta^3 x$, $\Sigma^2 x$, for ddx, $\Delta\Delta\Delta x$, $\Sigma\Sigma x$, &c. By the same analogy, since $\sin. x$, $\cos. x$, $\tan. x$, $\log. x$, &c. are merely *characteristic marks* to signify certain algebraic operations performed on the symbol x, (such as

$$\frac{\left\{1+\frac{1}{1}+\frac{1}{1.2}+\frac{1}{1.2.3}+\&c.\right\}^{x\sqrt{-1}} - \left\{1+\frac{1}{1}+\frac{1}{1.2}+\frac{1}{1.2.3}+\&c.\right\}^{-x\sqrt{-1}}}{2\sqrt{-1}};$$

&c.) we ought to write $\sin.^2 x$ for $\sin. \sin. x$, $\log.^3 x$ for $\log. \log. \log. x$, and so on. To apply this to the inverse functions, we have $\varphi^n \varphi^m (x) = \varphi^{n+m}(x)$. Hence if $m = -n$ $\varphi^n \varphi^{-n}(x) = \varphi^0(x) = x$, with the operation (φ) performed *no times* on it, or merely x, that is, $\varphi^{-n}(x)$ must be such a quantity that its nth (φ) shall be x, or in other words $\varphi^{-n}(x)$ must represent the nth *inverse* function. It frequently happens that a peculiar characteristic symbol is appropriated to the inverse function. Let it be ψ, then $\varphi^{-n}, x = \psi^n, x$, and $\varphi^n x = \varphi^{2n}, \varphi^{-n}, x = \varphi^n(\varphi^n, \psi^n, x)$, hence $\psi^n, \varphi^n, x = x$, and therefore $\psi^{-n} x = \psi^{-n} \psi^n \varphi^n, x = \varphi^{+n}, x$. For instance $d^{-n} V = \int^n V$, $d^0 V = V$. $-\Sigma^{-n}, x = \Delta^n x$. $a^n = 1$, with the operation of multiplying by a, n times performed on it, and $\therefore$ $a^{-n} = 1$, with the inverse operation so often performed on it, $= \frac{1}{a^n}$; $a^0 = 1$. Similarly $\sin.^{-1} x = \text{arc}(\sin. = x)$. $\cos.^{-2} x = \text{arc}(\cos.^2 = x)$ &c.—and if $c = 1 + \frac{1}{1} + \frac{1}{1.2} + \&c.$ $x = \log. c^x$ and $\therefore$ $c^x = \log.^{-1} x$, $c^{c^x} = \log.^{-2} x$, and $c^{c^{c \cdots (n)^x}} = \log.^{-n} x$, or the nth inverse logarithm of x. It is easy to carry on this idea, and its application to many very difficult operations in the higher branches will evince that it is somewhat more than a mere arbitrary contraction.

wherefore cos. $\alpha = \frac{\lambda + \lambda^{-1}}{2}$ and cos. $\alpha' = \frac{\lambda' + \lambda'^{-1}}{2}$.

Thus, $\lambda = c^{\alpha\sqrt{-1}}$ and $\lambda' = c^{\alpha'\sqrt{-1}}$, where $c = 1 + \frac{1}{1} + \frac{1}{1.2} + \frac{1}{1.2.3} +$ &c. Hence, $\lambda^n + \lambda^{-n} = 2 \cdot \cos. n\alpha$, and $\lambda'^n + \lambda'^{-n} = 2 \cdot \cos. n\alpha'$; $\lambda^n - \lambda^{-n} = 2\sqrt{-1} \cdot \text{cosin.}\, n\alpha$, $\lambda'^n - \lambda'^{-n} = 2\sqrt{-1} \cdot \sin. n\alpha'$. Consequently, if k be any arc

$1 - 2\lambda^n \cdot \cos. k + \lambda^{2n} = \lambda^n \left\{\lambda^n + \lambda^{-n} - 2 \cdot \cos. k\right\} = 4\lambda^n \cdot \sin. \left(\frac{k+n\alpha}{2}\right) \cdot \sin. \left(\frac{k-n\alpha}{2}\right)$.

In like manner

$$1 - 2\lambda'^n \cdot \cos. k + \lambda'^{2n} = 4\lambda'^n \cdot \sin. \left(\frac{k+n\alpha'}{2}\right) \cdot \sin. \left(\frac{k-n\alpha'}{2}\right).$$

We will now proceed to the application of these equations, and first, in Equation $\left\{1\right\}$ for θ substitute, successively, each of a series of angles

$$\theta_1;\ \theta_1 + \frac{2\pi}{n} = \theta_2;\ \theta_1 + \frac{4\pi}{n} = \theta_3;\ \ldots\ldots\ \theta_1 + \frac{2(n-1)\pi}{n} = \theta_n.$$

And let the resulting values of $r^{(1)}$ be

$$r_1^{(1)};\ r_2^{(1)};\ \ldots\ldots\ldots\ldots\ r_n^{(1)}$$

we have then

$$r_1^{(1)} \cdot r_2^{(1)} \ldots\ldots r_n^{(1)} = a^n \cdot \frac{(1-e^2)^n (1+\lambda^2)^n}{1 - 2\lambda^n \cdot \cos. n\theta_1 + \lambda^{2n}} \ldots\ldots\ldots\ldots \left\{1,1\right\}$$

for the product of the several denominators of the $r^{(1)}$ will be $\left\{1 - 2\lambda \cdot \cos. \theta_1 + \lambda^2\right\} \left\{1 - 2\lambda \cdot \cos. \theta_2 + \lambda^2\right\} \ldots\ldots \left\{1 - 2\lambda \cdot \cos. \theta_n + \lambda^2\right\} = 1 - 2\lambda^n \cdot \cos. n\theta_1 + \lambda^{2n}$ by Cotes's theorem.

This equation appears under an imaginary form when $e > 1$, but, since $\cos.^{-1} e$ is then a real angle, if we express it in α,

it will then be free from imaginary symbols; thus

$$\underset{1}{r^{(1)}} \ldots\ldots \underset{n}{r^{(1)}} = \frac{a^n(1-e^2)^n(\lambda+\lambda^{-1})^n}{\lambda^n+\lambda^{-n}-2\,.\cos.\,n\theta_1} = \frac{\left\{2p\,.\cos.\,\alpha\right\}^n}{4\,.\sin.\,\frac{n(\theta_1+\alpha)}{2}\,.\sin.\,\frac{n(\theta_1-\alpha)}{2}} \left\{1,2\right\}.$$

When $e = 1$, or the conic section is a parabola, $\lambda = 1$, and we find

$$\underset{1}{r^{(1)}} \ldots\ldots \underset{n}{r^{(1)}} = \frac{(2p)^n}{2\left\{1-\cos.\,n\theta_1\right\}} = (2p)^{n-1} \times \frac{p}{2}\,.\,\mathrm{cosec.}\left(\frac{n\theta_1}{2}\right)^2 \left\{1,3\right\}.$$

A result of such remarkable simplicity, as deserves a more particular enunciation. Let then, in the diagram, fig. 1, S represent the focus of a parabola A,P,Q, and, having drawn any line SP_1, make n angles P_1SP_2, P_2SP_3 .... P_nSP_1, about S, all equal to each other; draw the axis ASM, and make the angle $MSQ = n$ times MSP_1; and if L represent the latus rectum, we shall have

$$SP_1\,.\,SP_2 \ldots\ldots SP_n = L^{n-1}\,.\,SQ,$$

for, by the polar equation of the curve, $SQ = \frac{p}{2}\,.\,\mathrm{cosec.}\left(\frac{MSQ}{2}\right)^2.$

Thus, if SP_1 be coincident with SA, and n be odd, $\mathrm{cosec.}\,\frac{n\theta_1}{2} = 1$,

and $SP_1 \ldots SP_n = \frac{1}{4}L^n$ . . . . $\left\{1,4\right\}$;

but, if SP_1 be *perpendicular* to SA, and n still odd, $\mathrm{cosec.}\,\frac{n\theta_1}{2} = \sqrt{2}$. and $SP_1 \ldots SP_n = \frac{1}{2}L^n$ . . . . $\left\{1,5\right\}$.

If SP_1 be perpendicular to SA, but n of the form, $4m+2$

$SP_1 \ldots SP_n = \frac{1}{4}L^n$ . . . . $\left\{1,6\right\}$.

Lastly, if the angle $\underset{1}{MSP} = \frac{\pi}{3}$, we shall find, provided n be of the form $6m + 1$,

$$\underset{1}{SP} \ldots \underset{n}{SP} = L^n \quad . \quad . \quad . \quad . \quad . \quad . \quad \left\{1,7\right\}.$$

Let us now resume the general equation $\left\{1,1\right\}$ and first, let $\underset{1}{\theta} = o$, or, let one of the $r^{(1)}$ terminate in the second vertex, we have, for every value of n,

$$\underset{1}{r^{(1)}} \ldots\ldots \underset{n}{r^{(1)}} = p^n \cdot \frac{(1+\lambda^2)^n}{(1-\lambda^n)^2} = p^n \cdot \frac{(\lambda+\lambda^{-1})^n}{(\lambda^{\frac{n}{2}} - \lambda^{-\frac{n}{2}})^2} = -\frac{(2p)^n}{4} \cdot \frac{(\cos.\ \alpha)^n}{\left(\sin.\frac{n\alpha}{2}\right)^2}.$$

$\left\{1,8\right\}$.

2. Let $\cos.\ n\underset{1}{\theta} = -1$, and we obtain

$$\underset{1}{r^{(1)}} \ldots\ldots \underset{n}{r^{(1)}} = p^n \cdot \frac{(1+\lambda^2)^n}{(1+\lambda^n)^2} = p^n . = \frac{(\lambda+\lambda^{-1})^n}{(\lambda^{\frac{n}{2}} + \lambda^{-\frac{n}{2}})^2} = \frac{(2p)^n}{4} \cdot \frac{(\cos.\ \alpha)^n}{\left(\cos.\frac{n\alpha}{2}\right)^2}.$$

$\left\{1,9\right\}$.

This embraces all the cases where n is of the form $(2m + 1)$ $\frac{\pi}{\underset{1}{\theta}}$, and among the rest, when n is any odd number, and one of the $r^{(1)}$ terminates in the first vertex; when n is of the form $4m + 2$, and one of the $r^{(1)}$ perpendicular to the axis, &c.

3. Let $\cos.\ n\underset{1}{\theta} = o$; then

$$\underset{1}{r^{(1)}} \ldots\ldots \underset{n}{r^{(1)}} = p^n \cdot \frac{(1+\lambda^2)^n}{1+\lambda^{2n}} = p^n \cdot \frac{(\lambda+\lambda^{-1})^n}{\lambda^n+\lambda^{-n}} = \frac{(2p)^n}{2} \cdot \frac{(\cos.\ \alpha)^n}{\cos.\ n\alpha} \left\{1,10\right\}.$$

This includes the cases where n is of the form $(2m + 1)$. $\frac{\left(\frac{\pi}{2}\right)}{\theta}$, as for instance, where one of the $r^{(1)}$ is perpendicular to

the axis and n odd, or, one inclined at an angle $\frac{\pi}{4}$, and n of the form $4m+2$, or, lastly, where $\underset{1}{\theta} = \frac{\pi}{6}$ and $n = 6m+3$.

4. Let $\cos. n\underset{1}{\theta} = \frac{1}{2}$, then

$$\underset{1}{r}^{(1)} \ldots\ldots \underset{n}{r}^{(1)} = p^n \cdot \frac{(1+\lambda^2)^n}{1-\lambda^n+\lambda^{2n}} = p^n \cdot \frac{(1+\lambda^2)^n \cdot (1+\lambda^n)}{1+\lambda^{3n}} \cdot \left\{1,11\right\}.$$

This takes place whenever n is of the form $(6m+1) \cdot \dfrac{\left(\frac{\pi}{3}\right)}{\underset{1}{\theta}}$,

as when $\underset{1}{\theta} = \frac{\pi}{3}$ and $n = (6m+1)$; $\underset{1}{\theta} = \frac{\pi}{6}$, and $n = 12m + 2$, &c.

5. Let $\cos. n\underset{1}{\theta} = -\frac{1}{2}$, then

$$\underset{1}{r}^{(1)} \ldots\ldots \underset{n}{r}^{(1)} = p^n \cdot \frac{(1+\lambda^2)^n}{1+\lambda^n+\lambda^{2n}} = p^n \cdot \frac{(1+\lambda^2)^n \cdot (1-\lambda^n)}{1-\lambda^{3n}} \quad . \quad . \left\{1,12\right\}.$$

Here n must be of the form $\frac{2\pi}{3\underset{1}{\theta}} \cdot (3m+1)$, and if $\underset{1}{\theta} = \frac{2}{3}\pi$, n is of the form $3m+1$.

We will now proceed to our second equation $\left\{2\right\}$, and by an operation exactly similar to that from which we obtained the equation $\left\{1,1\right\}$, we shall find

$$\underset{1}{r}^{(2)} \ldots\ldots \underset{n}{r}^{(2)} = a^n (1+\lambda^2)^n \cdot \frac{1-2e^n \cdot \cos. n\underset{1}{\theta} + e^{2n}}{1-2\lambda^n \cdot \cos. n\underset{1}{\theta} + \lambda^{2n}} \cdot \quad . \quad . \quad . \left\{2,1\right\}$$

$$= a^n (\lambda + \lambda^{-1})^n \cdot \frac{1-2e^n \cdot \cos. n\underset{1}{\theta} + e^{2n}}{\lambda^n + \lambda^{-n} - 2 \cdot \cos. n\underset{1}{\theta}} = \frac{a^n\left\{1-2e^n \cdot \cos. n\underset{1}{\theta} + e^{2n}\right\}}{4 \cdot \sin. \frac{n(\theta+\alpha)}{2} \cdot \sin. \frac{n(\theta-\alpha)}{2}}.$$

This transformation in α, however, as it possesses no particular elegance in point of form, and much complexity, we shall henceforward omit, except in a few remarkable instances.

This equation, in the five particular cases just enumerated, gives

1. $n =$ any number, $\theta_1 = o$.

$$r^{(2)}_1 \ldots\ldots r^{(2)}_n = a^n (1 - e^n)^2 \cdot \frac{(1+\lambda^2)^n}{(1-\lambda^n)^2} \quad . \quad . \quad . \quad . \left\{2{,}2\right\}$$

2. $n = (2m + 1) \cdot \frac{\pi}{\theta_1}$.

$$r^{(2)}_1 \ldots\ldots r^{(2)}_n = a^n \cdot (1 + \lambda^2)^n \cdot \left(\frac{1+e^n}{1+\lambda^n}\right)^2 \quad . \quad . \quad . \left\{2{,}3\right\}$$

3. $n = (2m + 1) \cdot \frac{\pi}{2\theta_1}$.

$$r^{(2)}_1 \ldots\ldots r^{(2)}_n = a^n \cdot (1 + \lambda^2)^n \cdot \frac{1+e^{2n}}{1+\lambda^{2n}} \quad . \quad . \quad . \left\{2{,}4\right\}$$

4. $n = (6m + 1) \cdot \frac{\pi}{3\theta_1}$.

$$r^{(2)}_1 \ldots\ldots r^{(2)}_n = a^n \cdot (1 + \lambda^2)^n \cdot \left(\frac{1+\lambda^n}{1+e^n}\right) \cdot \left(\frac{1+e^{3n}}{1+\lambda^{3n}}\right) \quad . \quad . \left\{2{,}5\right\}$$

5. $n = (6m + 2) \cdot \frac{\pi}{3\theta_1}$.

$$r^{(2)}_1 \ldots\ldots r^{(2)}_n = a^n \cdot (1 + \lambda^2)^n \cdot \left(\frac{1-\lambda^n}{1-e^n}\right) \cdot \left(\frac{1-e^{3n}}{1-\lambda^{3n}}\right) \quad . \quad . \left\{2{,}6\right\}$$

Our third equation $\left\{3\right\}$, gives immediately

$$\frac{r^{(2)}_1 \ldots\ldots r^{(n)}_n}{r^{(1)}_1 \ldots\ldots r^{(1)}_n} = \frac{1 - 2e^n \cdot \cos. n\theta_1 + e^{2n}}{(1-e^2)^n} \quad . \quad . \quad . \quad . \left\{3{,}1\right\}$$

which, in the abovementioned cases, becomes

1. $$\frac{r^{(2)}_1 \ldots\ldots r^{(2)}_n}{r^{(1)}_1 \ldots\ldots r^{(1)}_n} = \frac{(1-e^n)^2}{(1-e^2)^n} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \left\{3{,}2\right\}$$

2. $$\frac{r^{(2)}_1 \ldots\ldots r^{(2)}_n}{r^{(1)}_1 \ldots\ldots r^{(1)}_n} = \frac{(1+e^n)^2}{(1-e^2)^n} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \left\{3{,}3\right\}$$

$$3.\quad \frac{\underset{1}{r}^{(2)} \ldots\ldots \underset{n}{r}^{(2)}}{\underset{1}{r}^{(1)} \ldots\ldots \underset{n}{r}^{(1)}} = \frac{1+e^{2n}}{(1-e^2)^n} \quad \ldots\ldots \quad \{3,4\}$$

$$4.\quad \frac{\underset{1}{r}^{(2)} \ldots\ldots \underset{n}{r}^{(2)}}{\underset{1}{r}^{(1)} \ldots\ldots \underset{n}{r}^{(1)}} = \frac{1+e^{3n}}{(1+e^n)(1-e^2)^n} \quad \ldots\ldots \quad \{3,5\}$$

$$5.\quad \frac{\underset{1}{r}^{(2)} \ldots\ldots \underset{n}{r}^{(2)}}{\underset{1}{r}^{(1)} \ldots\ldots \underset{n}{r}^{(1)}} = \frac{1-e^{3n}}{(1-e^n)(1-e^2)^n} \quad \ldots\ldots \quad \{3,6\}.$$

Let us now, instead of taking equal angles round the first focus, take a series of eccentric anomalies, in arithmetical progression

$$\underset{1}{\varpi};\ \underset{2}{\varpi} = \underset{1}{\varpi} + \frac{2\pi}{n};\ \ldots\ldots\ \underset{n}{\varpi} = \underset{1}{\varpi} + \frac{2(n-1)\pi}{n},$$

and let the resulting values of $r^{(1)}$ be, as before

$$\underset{1}{r}^{(1)};\ \underset{2}{r}^{(1)};\ \ldots\ldots\ \underset{n}{r}^{(1)}.$$

we get then, by the same process,

$$\underset{1}{r}^{(1)} \ldots\ldots \underset{n}{r}^{(1)} = a^n \cdot \frac{1-2\lambda^n \cdot \cos.\, n\underset{1}{\varpi} + \lambda^{2n}}{(1+\lambda^2)^n} \quad \ldots \quad \{4,1\}$$

This gives, in the five cases; 1st, when n is any number, and $\underset{1}{\varpi} = o$, (or one of the $r^{(1)}$ terminates in the first vertex); 2dly, when $n = (2m+1) \cdot \frac{\pi}{\underset{1}{\varpi}}$; 3dly, $n = (2m+1) \cdot \frac{\pi}{2\underset{1}{\varpi}}$; 4thly, $n = (6m+1) \cdot \frac{\pi}{3\underset{1}{\varpi}}$; and 5thly, $n = (6m+2) \cdot \frac{\pi}{3\underset{1}{\varpi}}$; the following equations

$$1.\quad \underset{1}{r}^{(1)} \ldots\ldots \underset{n}{r}^{(1)} = a^n \cdot \frac{\left(\lambda^{\frac{n}{2}} - \lambda^{-\frac{n}{2}}\right)^2}{(\lambda+\lambda^{-1})^n} \quad \ldots\ldots \quad \{4,2\}$$

$$2.\quad \underset{1}{r}^{(1)} \ldots\ldots \underset{n}{r}^{(1)} = a^n \cdot \frac{\left(\lambda^{\frac{n}{2}} + \lambda^{-\frac{n}{2}}\right)^2}{\lambda+\lambda^{-1})^n} \quad \ldots\ldots \quad \{4,3\}$$

$$3.\ r_1^{(1)} \ldots\ldots r_n^{(1)} = a^n \cdot \frac{\lambda^n + \lambda^{-n}}{(\lambda + \lambda^{-1})^n} \cdot \cdot \cdot \cdot \cdot \cdot \cdot \cdot \cdot \left\{4,4\right\}$$

$$4.\ r_1^{(1)} \ldots\ldots r_n^{(1)} = a^n \cdot \frac{\left(\lambda^{\frac{3n}{2}} + \lambda^{-\frac{3n}{2}}\right)}{(\lambda + \lambda^{-1}) \cdot \left(\lambda^{\frac{n}{2}} + \lambda^{-\frac{n}{2}}\right)} \cdot \cdot \cdot \cdot \cdot \left\{4,5\right\}$$

$$5.\ r_1^{(1)} \ldots\ldots r_n^{(1)} = a^n \cdot \frac{\left(\lambda^{\frac{3n}{2}} - \lambda^{-\frac{3n}{2}}\right)}{(\lambda + \lambda^{-1}) \cdot \left(\lambda^{\frac{n}{2}} - \lambda^{-\frac{n}{2}}\right)} \cdot \cdot \cdot \cdot \cdot \left\{4,6\right\}.$$

The Equation $\left\{5\right\}$ may be treated in the same manner, for the values of ϖ, being ϖ_1; $\varpi_1 + \frac{2\pi}{n}$; ...... $\varpi_1 + \frac{2(n-1)\pi}{n}$, those of $\pi - \varpi$ in an inverted order will be (if $\nu = -\left\{\varpi_1 + \frac{(n-2)\pi}{n}\right\}$)

$$\nu,\ \nu + \frac{2\pi}{n}, \ldots\ldots\ldots \nu + \frac{2(n-1)\pi}{n}.$$

Hence we find

$$r_1^{(2)} \ldots\ldots r_n^{(2)} = a^n \cdot \frac{1 - 2\lambda^n \cdot \cos. n\nu + \lambda^{2n}}{(1 + \lambda^2)^n},$$

or, since $\cos. \nu = \cos. n\,(\pi + \varpi_1)$

$$r_1^{(2)} \ldots\ldots r_n^{(2)} = a^n \cdot \frac{1 - 2\lambda^n \cdot \cos. n\,(\pi + \varpi_1) + \lambda^{2n}}{(1 + \lambda^2)^n} \cdot \cdot \cdot \cdot \left\{5,1\right\}.$$

As this case, however, is manifestly similar to that of $\left\{4,1\right\}$, we shall pursue it no farther.

The 6th of the equations, in page 9, offers, however, some results worthy of consideration. By treating it like the rest, it becomes

$$\frac{r_1^{(1)} \ldots\ldots r_n^{(1)}}{r_1^{(2)} \ldots\ldots r_n^{(2)}} = \frac{1 - 2\lambda^n \cdot \cos. n\varpi_1 + \lambda^{2n}}{1 - 2\lambda^n \cdot \cos. n\,(\pi + \varpi_1) + \lambda^{2n}} \cdot \cdot \cdot \cdot \cdot \left\{6,1\right\}.$$

1. When $\varpi_1 = o$, it becomes

$$\cdots \frac{(1-\lambda^n)^2}{1-2\lambda^n \,.\, \cos.\,(n\pi)+\lambda^{2n}}$$

If then, n be even, this is equal to unity, as it evidently ought, but if odd

$$\frac{\underset{1}{r}^{(1)}\ldots\ldots\underset{n}{r}^{(1)}}{\underset{1}{r}^{(2)}\ldots\ldots\underset{n}{r}^{(2)}} = \left(\frac{1-\lambda^n}{1+\lambda^n}\right)^2 = -\left(\tan.\frac{n\alpha}{2}\right)^2 \quad . \quad . \quad . \quad . \quad \left\{6,2\right\}.$$

2. Let $\underset{1}{\varpi} = \frac{\pi}{3}$, and $n = 6m+1$, and we find

$$\frac{\underset{1}{r}^{(1)}\ldots\ldots\underset{n}{r}^{(1)}}{\underset{1}{r}^{(2)}\ldots\ldots\underset{n}{r}^{(2)}} = \left(\frac{1+\lambda^{3n}}{1-\lambda^{3n}}\right) . \left(\frac{1-\lambda^n}{1+\lambda^n}\right) = \tan.\frac{n\alpha}{2}.\,\text{cotan.}\frac{3n\alpha}{2} \quad \left\{6,3\right\}.$$

We come now to our 7th Equation, which will afford us results, more complicated indeed, yet equally interesting. By applying the same method of transformation to it, we shall find, (supposing $\underset{1}{\varphi}, \underset{2}{\varphi}, \ldots\ldots \underset{n}{\varphi} = \underset{1}{\varphi} + \frac{2(n-1)\pi}{n}$, to be written for φ, and $\underset{1}{\text{R}}, \underset{2}{\text{R}}, \ldots\ldots \underset{n}{\text{R}}$ to denote the resulting values of R)

$$\underset{1}{\text{R}}\ldots\ldots\underset{n}{\text{R}} = a^n . \frac{(1+\lambda^2)^n\,(1-e^2)^{\frac{n}{2}}}{\left\{1-2\lambda^n . \cos.\, n\underset{1}{\varphi} + \lambda^{2n}\right\}^{\frac{1}{2}} . \left\{1-2\lambda^n . \cos.\, n\,(\pi+\underset{1}{\varphi}) + \lambda^{2n}\right\}^{\frac{1}{2}}}.$$
$$\left\{7,1\right\}.$$

1. If n be even, $\cos.\, n\underset{1}{\varphi} = \cos.\, n\,(\pi+\underset{1}{\varphi})$ and, since $1-e^2 = \left\{\frac{1-\lambda^2}{1+\lambda^2}\right\}^2$, this becomes

$$\underset{1}{\text{R}}\ldots\ldots\underset{n}{\text{R}} = a^n . \frac{(1-\lambda^2)^n}{1-2\lambda^n . \cos.\, n\underset{1}{\varphi}+\lambda^{2n}} \quad . \quad . \quad . \quad . \left\{7,2\right\}.$$

2. If n be odd, $\cos.\, n\underset{1}{\varphi} = -\cos.\, n\,(\pi+\underset{1}{\varphi})$, whence

$$\underset{1}{\text{R}}\ldots\ldots\underset{n}{\text{R}} = a^n . \frac{(1-\lambda^2)^n}{\left\{1-2\lambda^n . \cos.\, n\underset{1}{\varphi}+\lambda^{2n}\right\}^{\frac{1}{2}} . \left\{1+2\lambda^n . \cos.\, n\underset{1}{\varphi}+\lambda^{2n}\right\}^{\frac{1}{2}}}$$

$$= a^n \cdot \frac{(1-\lambda^2)^n}{\left\{1-2\lambda^{2n}(2\,.\,\cos.\,n\underset{1}{\phi}^2-1)+\lambda^{4n}\right\}^{\frac{1}{2}}}, \text{ or}$$

$$\underset{1}{R}\ldots\ldots\underset{n}{R} = a^n \cdot \frac{(1-\lambda^2)^n}{\left\{1-2\lambda^{2n}\,.\,\cos.\,2n\underset{1}{\phi}+\lambda^{4n}\right\}^{\frac{1}{2}}} \quad \ldots\ldots \quad \{7,3\}.$$

Let $\underset{1}{\phi} = o$, or, let the extremity of one of the R lie in the principal vertex, If n be even

$$\underset{1}{R}\ldots\ldots\underset{n}{R} = a^n \cdot \frac{(1-\lambda^2)^n}{(1-\lambda^n)^2} = a^n \cdot \frac{(\sin.\,\alpha)^n}{(\sin.\,\frac{n}{2}\,\alpha)^2} \quad \ldots\ldots \quad \{7,4\}.$$

If odd,

$$\underset{1}{R}\ldots\ldots\underset{n}{R} = a^n \cdot \frac{(1-\lambda^2)^n}{1-\lambda^{2n}} = a^n \cdot \frac{(\sin.\,\alpha)^n}{\sin.\,n\alpha} \quad \ldots\ldots \quad \{7,5\}.$$

Let $\underset{1}{\phi} = \frac{\pi}{2}$, and $n = 4m + 2$, In this case, two out of the R are at right angles to the axis, and

$$\underset{1}{R}\ldots\ldots\underset{n}{R} = a^n \cdot \frac{(1-\lambda^2)^n}{(1+\lambda^n)^2} \quad \ldots\ldots \quad \{7,6\}.$$

Again, let *one only* of the R be perpendicular to the axis, and

$$\underset{1}{R}\ldots\ldots\underset{n}{R} = a^n \cdot \frac{(1-\lambda^2)^n}{1+\lambda^{2n}} \quad \ldots\ldots \quad \{7,7\}.$$

Here n is of course odd.

Next, let one of the R be inclined at an angle $\frac{\pi}{4}$, to the axis. If $n = 4m + 2$,

$$\underset{1}{R}\ldots\ldots\underset{n}{R} = a^n \cdot \frac{(1-\lambda^2)^n}{1+\lambda^{2n}} \quad \ldots\ldots \quad \{7,8\},$$

and it is curious to observe, that this expression is the same function of a, λ, n, as that of $\{7,7\}$.

If n be of the form $2m + 1$,

$$\underset{1}{R}\ldots\ldots\underset{n}{R} = a^n \cdot \frac{(1-\lambda^2)^n}{\sqrt{1+\lambda^{4n}}} \quad \ldots\ldots \quad \{7,9\}.$$

Lastly, let n be of the form $6m + 2$, and $\underset{1}{\phi} = \frac{\pi}{3}$, then

$$\underset{1}{R}\ldots\ldots\underset{n}{R} = a^n \cdot \frac{(1-\lambda^2)^n(1-\lambda^n)}{(1-\lambda^{2n})} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad \{7,10\}:$$

but, if $n = 6m + 1$,

$$\underset{1}{R}\ldots\ldots\underset{n}{R} = a^n \cdot \frac{(1-\lambda^2)^n}{\sqrt{(1+\lambda^{2n}+\lambda^{4n})}} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad \{7,11\}.$$

These are always imaginary expressions when $e > 1$ and n odd. In fact, R, in the hyperbola, must be written

$$\frac{a\sqrt{e^2-1}}{\sqrt{e^2 \cdot \cos.\varphi^2-1}} \text{ instead of } \frac{a\sqrt{1-e^2}}{\sqrt{1-e^2 \cdot \cos.\varphi^2}}$$

now $\sqrt{e^2-1} = e \cdot \frac{1-\lambda'^2}{1+\lambda'^2}$. Thus, this expression, like the rest, is easily transformed, in functions of a, λ, λ', the λ' is now real, and the part involving λ will always be of the form $f(\lambda^m \pm \lambda^{-m})$, and therefore readily expressed in trigonometrical functions.

Before we proceed farther, it will be necessary to premise a transformation of COTES's formula, which we shall have occasion to make use of. It is as follows:

$$\left.\begin{array}{l}
\sin.(A+B) \cdot \sin.(A-B) = 2^{2n-2} \cdot P \cdot Q, \text{ where} \\
P = \sin.\left(\frac{A+B}{n}\right) \cdot \sin.\left(\frac{\pi+(A+B)}{n}\right) \cdot \sin.\left(\frac{2\pi+(A+B)}{n}\right) \ldots\ldots \\
\sin.\left(\frac{(n-1)\pi+(A+B)}{n}\right) \\
Q = \sin.\left(\frac{A-B}{n}\right) \cdot \sin.\left(\frac{\pi+(A-B)}{n}\right) \cdot \sin.\left(\frac{2\pi+(A-B)}{n}\right) \ldots\ldots \\
\sin.\left(\frac{(n-1)\pi+(A-B)}{n}\right)
\end{array}\right\}(a)$$

The demonstration is extremely simple,

$$1-2x^n \cdot \cos.a + x^{2n} = \left(1-2x \cdot \cos.\frac{a}{n}+x^2\right)\left(1-2x \cdot \cos.\frac{a+2\pi}{n}+x^2\right)\ldots\ldots\left(1-2x \cdot \cos.\frac{a+2(n-1)\pi}{n}+x^2\right),$$

or dividing by x^n

$$x^n + x^{-n} - 2 \cdot \cos.a = \left(x+x^{-1}-2 \cdot \cos.\frac{a}{n}\right)\ldots\ldots$$

$\left(x + x^{-1} - 2 \,.\, \cos. \frac{a+2(n-1)\pi}{n}\right)$.

Let $x + x^{-1} = 2 \,.\, \cos. c$; then $x^n + x^{-n} = 2 \,.\, \cos. nc$, and $\cos. nc - \cos. a = 2^{n-1} \left(\cos. c - \cos. \frac{a}{n}\right) \ldots\ldots \left(\cos. c - \cos. \frac{a+2(n-1)\pi}{n}\right)$, that is (by the formula $\cos. x - \cos. y = -2 \,.\, \sin. \frac{x+y}{2} \,.\, \sin. \frac{x-y}{2}$) $. \sin. \left(\frac{a+nc}{2}\right) . \sin. \left(\frac{a-nc}{2}\right) = 2^{2n-2}$

$$. \sin. \tfrac{1}{2} \left(\frac{a}{n} + c\right) . \sin. \tfrac{1}{2} \left(\frac{a+2\pi}{n} + c\right) . \&c.$$

$$. \sin. \tfrac{1}{2} \left(\frac{a}{n} - c\right) . \sin. \tfrac{1}{2} \left(\frac{a+2\pi}{n} - c\right) . \&c.$$

Let $\frac{a+nc}{2} = A + B$, and $\frac{a-nc}{2} = A - B$, and by substitution, the formula under consideration results.

This immediately gives the following

$$\left.\begin{array}{l}\cos. (A + B) \,.\, \cos. (A - B) = 2^{2n-2} \,.\, P \,.\, Q, \text{ where} \\ P = \sin. \left\{\frac{\left(\frac{\pi}{2}\right) - (A+B)}{n}\right\} . \sin. \left\{\frac{3\left(\frac{\pi}{2}\right) - (A+B)}{n}\right\} \ldots\ldots \\ \sin. \left\{\frac{(2n-1) . \left(\frac{\pi}{2}\right) - (A+B)}{n}\right\} \\ Q = \sin. \left\{\frac{\left(\frac{\pi}{2}\right) - (A-B)}{n}\right\} . \sin. \left\{\frac{3\left(\frac{\pi}{2}\right) - (A-B)}{n}\right\} \ldots\ldots \\ \sin. \left\{\frac{(2n-1) \left(\frac{\pi}{2}\right) - (A-B)}{n}\right\}\end{array}\right\}(b)$$

and also,

$$\left.\begin{array}{l}\cos. (A + B) \,.\, \sin. (A - B) = 2^{2n-2} \,.\, P \,.\, Q, \text{ where} \\ P = \sin. \left\{\frac{\left(\frac{\pi}{2}\right) - (A+B)}{n}\right\} . \sin. \left\{\frac{3\left(\frac{\pi}{2}\right) - (A+B)}{n}\right\} \ldots\ldots \\ \sin. \left\{\frac{(2n-1) \left(\frac{\pi}{2}\right) - (A+B)}{n}\right\} \\ Q = \sin. \left(\frac{A-B}{n}\right) . \sin. \left(\frac{\pi + (A-B)}{n}\right) \ldots . \sin. \left(\frac{(n-1)\pi + (A-B)}{n}\right).\end{array}\right\}(c)$$

Let $B = o$, and (a) gives

$$\sin. A = 2^{n-1} . \sin. \frac{A}{n} . \sin. \frac{\pi + A}{n} . \sin. \frac{2\pi + A}{n} \ldots \sin. \frac{(n-1)\pi + A}{n}. \quad (d)$$

If $A = \frac{\pi}{2}$, this becomes

$$1 = 2^{n-1} . \sin. \frac{\pi}{2n} . \sin. \frac{3\pi}{2n} . \sin. \frac{5\pi}{2n} \ldots\ldots \sin. \frac{(2n-1)\pi}{2n}. \quad (e)$$

(b) gives by making $B = o$,

$$\cos. A = 2^{n-1} . \sin. \frac{\left(\frac{\pi}{2}\right) - A}{n} . \sin. \frac{3\left(\frac{\pi}{2}\right) - A}{n} \ldots \sin. \frac{(2n-1)\left(\frac{\pi}{2}\right) - A}{n}. \quad (f)$$

If $A = \frac{\pi}{3}$, this gives

$$1 = 2^{n} . \sin. \frac{\pi}{6n} . \sin. \frac{7\pi}{6n} . \sin. \frac{13\pi}{6n} . \sin. \frac{19\pi}{6n} \ldots\ldots \sin. \frac{(6n-5)\pi}{6n}. \quad (g)$$

Equation (c) divided by (b) gives, (putting A, for $A - B$).

$$\tan. A = \frac{\sin. \frac{A}{n} . \sin. \frac{\pi + A}{n} . \sin. \frac{2\pi + A}{n} \ldots\ldots \sin. \frac{(n-1)\pi + A}{n}}{\sin. \left\{ \frac{\left(\frac{\pi}{2}\right) - A}{n} \right\} . \sin. \left\{ \frac{3\left(\frac{\pi}{2}\right) - A}{n} \right\} \ldots \sin. \left\{ \frac{(2n-1)\left(\frac{\pi}{2}\right) - A}{n} \right\}}. \quad (h)$$

But, to return from this digression, let us take Equation $\{8\}$, and putting it into this form

$$\rho = \frac{2p . (1 + \lambda^2)^2 . \sin. \left(\frac{\pi}{2} - \psi\right)}{(1 - 2\lambda . \cos. \psi + \lambda^2)\,(1 - 2\lambda . \cos. (\pi - \psi) + \lambda^2)}$$

for ψ substitute each of a series of angles, n in number

$$\psi_1 ; \ \psi_1 + \frac{\pi}{n} ; \ \psi_1 + \frac{2\pi}{n} ; \ldots\ldots \psi_1 + \frac{(n-1)\pi}{n},$$

and let $\rho_1, \rho_2, \ldots\ldots \rho_n$ be the resulting values of ρ.

The values of $\sin. \left(\frac{\pi}{2} - \psi\right)$ in an inverted order, are (if $\nu = -\left\{\psi_1 + \frac{n-2}{n} . \left(\frac{\pi}{2}\right)\right\}$) $\sin. \nu$; $\sin. \left(\nu + \frac{\pi}{n}\right)$; $\sin. \left(\nu + \frac{2\pi}{n}\right)$; $\ldots\ldots \sin. \left(\nu + \frac{n-1}{n}\pi\right)$, and their product, $= \sin. \frac{n\nu}{n} . \sin. \frac{\pi + n\nu}{n} \ldots\ldots \sin. \frac{(n-1)\pi + n\nu}{n} = \frac{\sin. n\nu}{2^{n-1}}$ (by Equation (d)) $= \frac{\sin. n\left(\frac{\pi}{2} + \psi_1\right)}{2^{n-1}}$.

Again, the values of ψ being $\underset{1}{\psi}, \ldots \ldots \underset{1}{\psi} + \frac{(n-1)\pi}{n}$, those of $\pi + \psi$, or $2\pi - (\pi - \psi)$ will be

$$\underset{1}{\psi} + \frac{n\pi}{n}, \underset{1}{\psi} + \frac{(n+1)\pi}{n}; \ldots . \underset{1}{\psi} + \frac{(2n-1)\pi}{n}.$$

Now, cos. $2\pi - (\pi - \psi) =$ cos. $(\pi - \psi)$. Hence, the product of all the denominators of $\underset{1}{\rho}, \underset{2}{\rho}$, &c. will be $(1 - 2\lambda \,.\, \cos.\, \underset{1}{\psi} + \lambda^2)(1 - 2\lambda \,.\, \cos.\,(\underset{1}{\psi} + \frac{\pi}{n}) + \lambda^2) \ldots (1 - 2\lambda \,.\, \cos.\,(\underset{1}{\psi} + \frac{(n-1)\pi}{n}) + \lambda^2)$. $\cos.\,(\underset{1}{\psi} + \frac{\pi}{n}) + \lambda^2) \ldots (1 - 2\lambda \,.\, \cos.\,(\underset{1}{\psi} + \frac{(2n-1)\pi}{n}) + \lambda^2) =$, by Cotes's formula

$$1 - 2\lambda^{2n} \,.\, \cos.\, 2n\underset{1}{\psi} + \lambda^{4n}.$$

Thus we have, combining these separate processes,

$$\left.\begin{aligned} &\underset{1}{\rho} \,.\, \underset{2}{\rho} \ldots \underset{n}{\rho} = 2a^n (1 - e^2)^n \,.\, (1 + \lambda^2)^{2n} \,.\, \frac{\sin.\, n\left(\frac{\pi}{2} + \underset{1}{\psi}\right)}{1 - 2\lambda^{2n} \,.\, \cos.\, 2n\underset{1}{\psi} + \lambda^{4n}} \\ &= 2p^n \,.\, \frac{(1+\lambda^2)^{2n} \,.\, \sin.\, n\left(\frac{\pi}{2} + \underset{1}{\psi}\right)}{1 - 2\lambda^{2n} \,.\, \cos.\, 2n\underset{1}{\psi} + \lambda^{4n}} = 2a^n \,.\, \frac{(1-\lambda^2)^{2n} \,.\, \sin.\, n\left(\frac{\pi}{2} + \underset{1}{\psi}\right)}{1 - 2\lambda^{n} \,.\, \cos.\, 2n\underset{1}{\psi} + \lambda^{4n}} \end{aligned}\right\} \{\quad\}$$

1. Let n be any number of the form $4m + 1$; then

$$\underset{1}{\rho} \ldots \underset{n}{\rho} = 2p^n \,.\, \frac{(1+\lambda^2)^{2n} \,.\, \cos.\, n\underset{1}{\psi}}{1 - 2\lambda^{2n} \,.\, \cos.\, 2n\underset{1}{\psi} + \lambda^{4n}} \quad . \quad . \quad . \quad . \quad . \quad \{8,2\}.$$

If now $\underset{1}{\psi} = o$, or one of the ρ coincide with the transverse axis,

$$\underset{1}{\rho} \ldots \underset{n}{\rho} = \frac{2p^n (1+\lambda^2)^{2n}}{(1-\lambda^{2n})^2} = 2a^n \,.\, \frac{(1-\lambda^2)^{2n}}{(1-\lambda^{2n})^2} \quad . \quad . \quad . \quad . \quad . \quad \{8,3\}.$$

Let, next, $\underset{1}{\psi} = \frac{\pi}{4}$ $\{n$ continuing $= 4m + 1\}$ and,

$$\underset{1}{\rho} \ldots \underset{n}{\rho} = \pm p^n \sqrt{2} \,.\, \frac{(1+\lambda^2)^{2n}}{1 + \lambda^{4n}} \quad . \quad . \quad . \quad . \quad \{8,4\}.$$

In the parabola, $\lambda = 1$. Hence in this case,

$$\underset{1}{\rho} \cdots \underset{n}{\rho} = \pm (2L)^n \cdot \sqrt{\tfrac{1}{2}} \quad . \quad . \quad . \quad . \quad \{8,5\}.$$

In fig. 2, let ASM be the axis of a parabola, BAC a tangent at the vertex, S the focus—bisect the angle BAM by $\underset{1}{AP}$, and draw $4m$ other lines $\underset{2}{AP}$, $\underset{3}{AP}$, ... $\underset{n}{AP}$, so that if $\underset{1}{PA}$, $\underset{2}{PA}$, &c. be produced through A, this system of lines shall make equal angles around A, then, neglecting the sign

$$\underset{1}{AP} \cdot \underset{2}{AP} \ldots\ldots \underset{n}{AP} = (2L)^n \cdot \sqrt{\tfrac{1}{2}}.$$

If $n = 4m + 3$, the resulting value of $\underset{1}{\rho} \ldots \underset{n}{\rho}$ is of the same form as $\{8,2\}$ with the exception of a different sign.

If n be of the form $4m$, or $4m + 2$,

$$\underset{1}{\rho} \ldots\ldots \underset{n}{\rho} = \pm \frac{2a^n (1-\lambda^2)^{2n} \cdot \sin. n\underset{1}{\psi}}{1 - 2\lambda^{2n} \cdot \cos. 2n\underset{1}{\psi} + \lambda^{4n}} \quad . \quad . \quad . \quad . \quad \{8,6\},$$

where the sign $+$, or $-$, is to be used, according as n is of the former, or latter form.

When $n = 4m + 2$, if $\underset{1}{\psi} = \frac{\pi}{4}$, this becomes

$$\underset{1}{\rho} \ldots\ldots \underset{n}{\rho} = \pm 2p^n \cdot \frac{(1+\lambda^2)^{2n}}{(1+\lambda^{2n})^2} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad \{8,7\},$$

and when $\lambda = 1$.

$$\underset{1}{\rho} \ldots\ldots \underset{n}{\rho} = \pm \frac{(2L)^n}{2} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad \{8,8\}.$$

Thus, in the last mentioned construction, if the number of lines be $4m + 2$, instead of $4m + 1$

$$\underset{1}{AP} \cdot \underset{2}{AP} \ldots\ldots \underset{n}{AP} = \frac{(2L)^n}{2}.$$

The ninth of our primitive equations gives, if the angles

ϖ_1; $\varpi_2 = \varpi_1 + \frac{2\pi}{n}$; .... $\varpi_n = \varpi_1 + \frac{2(n-1)\pi}{n}$, be written for ϖ, and for θ $\left\{\text{a function of } \varpi\text{, given by the equation}\right.$

$$\left.\cot. \tfrac{1}{2}\theta = \frac{1+\lambda}{1-\lambda} . \tan. \tfrac{1}{2}\varpi.\right\}$$

be put θ_1; .... θ_n, the results of that substitution

$$\cos. \theta_1 . \cos. \theta_2 \ldots \cos. \theta_n = e^{-n} . \left(\frac{\lambda}{\lambda'}\right)^n . \frac{1 - 2'\lambda^n . \cos. n\varpi_1 + \lambda'^{2n}}{1 - 2\lambda^n . \cos. n\varpi_1 + \lambda^{2n}} . \{9,1\}.$$

If $\varpi_1 = o$, whatever be n,

$$\cos. \theta_1 \ldots \cos. \theta_n = e^{-n} . \left(\frac{\lambda}{\lambda'}\right)^n . \left(\frac{1-\lambda'^n}{1-\lambda}\right)^2 = e^{-n} . \left\{\frac{\lambda'^{\frac{n}{2}} - \lambda'^{-\frac{n}{2}}}{\lambda^{\frac{n}{2}} - \lambda^{-\frac{n}{2}}}\right\}^2 . \{9,2\}.$$

If $n = (2m+1)\ \frac{\pi}{\varpi_1}$

$$\cos. \theta \ldots . \cos. \theta_n = e^{-n} . \left\{\frac{\lambda'^{\frac{n}{2}} + \lambda'^{-\frac{n}{2}}}{\lambda^{\frac{n}{2}} + \lambda^{-\frac{n}{2}}}\right\}^2 \quad \ldots\ldots \quad \{9,3\}.$$

If $n = (2m+1) . \frac{\left(\frac{\pi}{2}\right)}{\varpi_1}$,

$$\cos. \theta_1 \ldots . \cos. \theta_n = e^{-n} . \frac{\lambda'^n + \lambda'^{-n}}{\lambda^n + \lambda^{-n}} \quad \ldots\ldots \quad \{9,4\}.$$

If $n = (6m+1) . \frac{\left(\frac{\pi}{3}\right)}{\varpi_1}$;

$$\cos\ \theta_1 \ldots . \cos. \theta_n = e^{-n} . \left(\frac{\lambda^{\frac{n}{2}} + \lambda^{-\frac{n}{2}}}{\lambda'^{\frac{n}{2}} + \lambda'^{-\frac{n}{2}}}\right) . \left(\frac{\lambda'^{\frac{3n}{2}} + \lambda'^{-\frac{3n}{2}}}{\lambda^{\frac{3n}{2}} + \lambda^{-\frac{3n}{2}}}\right) \{9,5\}.$$

If $n = (6m+2) . \frac{\left(\frac{\pi}{3}\right)}{\varpi_1}$,

$$\cos. \theta_1 \ldots . \cos. \theta_n = e^{-n} . \left(\frac{\lambda^{\frac{n}{2}} - \lambda^{-\frac{n}{2}}}{\lambda'^{\frac{n}{2}} - \lambda'^{-\frac{n}{2}}}\right) . \left(\frac{\lambda'^{\frac{3n}{2}} - \lambda'^{-\frac{3n}{2}}}{\lambda^{\frac{3n}{2}} - \lambda^{-\frac{3n}{2}}}\right) \{9,6\}.$$

These theorems, however simple their algebraic expressions, it must immediately be seen, become for the most part complicated and unintelligible when geometrically enunciated. They are indeed (if we may in any case be allowed to consider a curve as unidentified with its equation) properties rather of the equations of the conic sections, than of the curves themselves,—of a limited number of disjoined points determined according to a certain law, rather than a series of consecutive ones composing a line.

JOHN F. W. HERSCHEL.

Slough, Oct. 6, 1812.

Fig. 1.

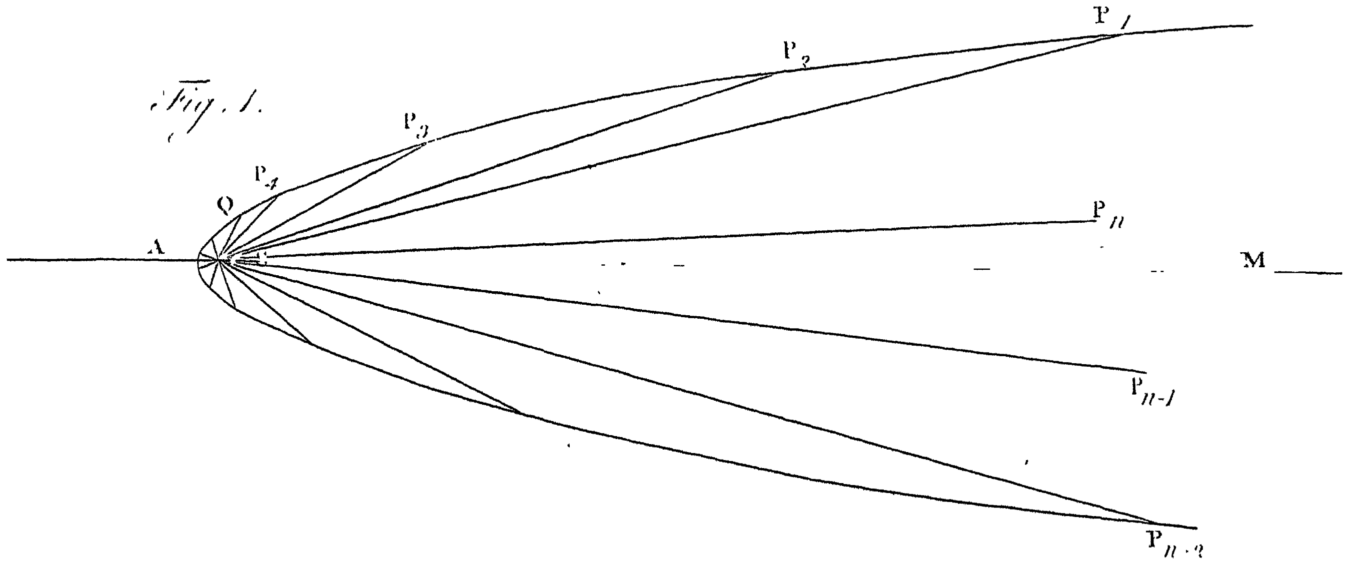
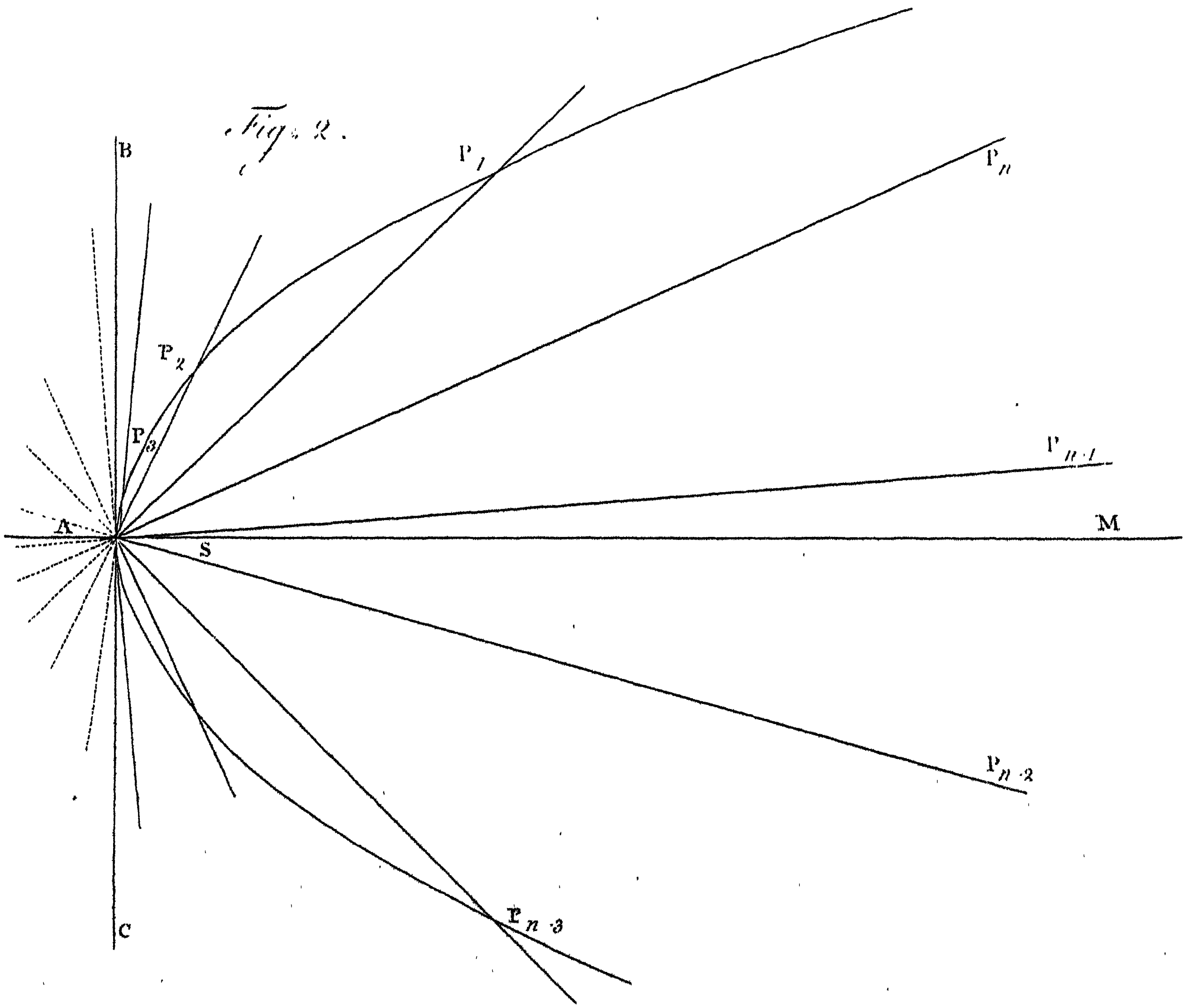


Fig. 2.



J. Basire sculp.

III. *Observation of the Summer Solstice,* 1812, *at the Royal Observatory.* *By* John Pond, *Esq. Astronomer Royal, F. R. S.*

Read November 12, 1812.

As it is intended that a minute description of the mural circle lately erected at the Royal Observatory, when completely finished, shall be laid before the Royal Society, I think it unnecessary to accompany this communication with any other remarks on its construction, than such as are absolutely necessary to render the annexed observations intelligible; being only anxious to take an early opportunity of transmitting the result of the observations of the sun, made at the last summer solstice. For, notwithstanding the instrument was at that time in a very unfinished and imperfect state, I have reason to think, that the observations made with it were much more exact than could have been made with any other instrument hitherto constructed: the uncertainty of the result, as far as the instrument itself is concerned, does not, I believe, exceed a small fraction of a second; but I think it necessary to offer a few remarks on the elements of the computation, by which the result is obtained.

The distance of the sun from the pole at the moment of the solstice, may either be considered as a simple arc, or as composed of two others, namely, the distance of the sun from the zenith, and the distance of the zenith from the pole. From the construction of instruments which take their point of departure from the position of a plumb-line or level, it is a general

method to measure these two arcs separately; that is to say, the zenith distance of the sun is considered as the immediate object of investigation, and the remaining arc, or as it is usually termed, the co-latitude of the place, (the knowledge of which is presumed) is added to complete the polar distance. But in whatever way we consider the subject, it is evident that the sole object of the practical astronomer is to obtain by some mechanical means the measure of this total arc.

The mural circle at Greenwich has neither level nor plumb-line, nor do I conceive that the least advantage could be derived from the application of either: its principle enables the observer to determine this total arc without any intermediate or zenith point, hence the co-latitude, the knowledge of which is so essential from the construction of other instruments, is in this case rather a question of curiosity than of absolute necessity.

I find it, however, convenient (for reasons which I hope to have an opportunity of stating more at large in a future Paper) to assume an intermediate point, which I call the zenith point, without being at all anxious to know whether it is really so or not; because I find myself possessed of the means of determining the position of this *imaginary* point of departure, on the instrument, to within the tenth of a second, a degree of precision, which I apprehend no level or plumb-line can ever be expected to equal.

From this point of departure, I measure the distance of the sun to the southward, and of the pole to the northward, and the sum of these two measures is evidently the north polar distance of the sun, which in every method is the ultimate object to be attained.

I have, as an example, annexed a computation of the same

solstice obtained by direct measurement, from the pole, without the aid of the intermediate point above mentioned; and it will be seen that the results do not in this case differ above a quarter of a second from each other.

There is indeed no other difference between these two methods but that, in the former case, the part of the arc Z P is obtained rather more accurately, by a great number of observations both before and after the solstice, than could be done in the short interval of time in which the solstice itself is observed.

By sixty observations of γ draconis, of which only three differ so much as 1″ from the mean, it appears that the zenith point which I have assumed is 2′, 18″,64, south of the mean position of γ draconis for the beginning of the year 1812, which is the same quantity that is found by the observations with the zenith sector, 1811.

Mr. TROUGHTON is now occupied in making a zenith sector upon a new and very simple principle, with which I have no doubt that this distance may be determined with a much greater degree of precision.

Astronomers will immediately perceive that this arc, however accurately it may be mechanically determined, must inevitably be subject to whatever uncertainty still exists upon the question of astronomical refractions; the instrument not having been erected long enough to remove this uncertainty, I have for the present employed Dr. BRADLEY's refractions, such as they have been used for many years in this Observatory. Such alterations in this part of the calculation may easily be made in future, as the advanced state of the science may require.

1812	Bar.	Ther. in.	Ther out	Ref.		Observations as given by the Instrument.	Position of the Zero Point, or Equation, to be applied to obtain the apparent N. P. D.	Equation to reduce the observed N. P. D to Zenith distance, or Position of Zenith Point on the Circle.	Semi diameter of ⊙ 1″ greater than in the Nautical Almanack.	Reduction to the Solstice.	Solsticial Zenith. Distance with Parallax.	N. P. D. deduced by direct measurement from Polaris.
June				′ ″								
12	29,94	64	70	0,30,3	⊙ UL	66,33,33,0	12,95	38,31,34,10	15,47,2	0,17,15,4	28, 1, 1,0	66,32,21,4
14	29,76	63	71	0,29,4	⊙ UL	66,26 48,5	12,95	34,10	15,47,2	10,32,4	0,58,6	18,9
18	29,77	58	62	0,29,6	⊙ UL	66,18,20.2	12.95	34,10	15,46,8	2, 2,6	0,59,9	20,2
19	29,31	59	61	0,29,7	⊙ LL	65,48,44,6	10,04	38,31,31,20	15,46,8	0,57,0	0,5[illegible],3	19,9
20	29,28	57	60½	0,29,2	⊙ UL	66,16 31,4	10,04	31,20	15,46,8	0,16,3	0,59 9	20.5
23	29,78	56	61	0,30,4	⊙ LL	66,48,43,5	23,59	38,31,44,74	15,46,6	0,42,7	0,59 7	21,3
24	29,88	58	62	0,29,8	⊙ UL	66,18,11,3	23,59	44,74	15,46,6	1,41,1	1, 1,9	23,3
25	29,86	58	65	0,30,4	⊙ LL	66,51, 5,5	23,59	44,74	15,46,6	3, 4,2	1, 0,4	21,8
27	29,81	56	58	0,29,7	⊙ UL	66,23,33,5	23,59	44,74	15,46,6	7, 4,4	1, 0,7	22,[illegible]
28	29,78	54	54	0,30,7	⊙ LL	66,57,41,5	23.59	44,74	15,46,6	9,41,4	0,59,5	20,8
29	30,05	58	64	0,30,1	⊙ UL	66,29,11,4	23,59	44,74	15,46,6	12,43,0	1, 0,2	21,7
30	29,90	58	63	0,30,7	⊙ LL	67, 4, 9,5	23,59	44,74	15,46,6	16, 8,9	1, 0,1	21,4

Mean of 12 Observations - - - - - -	28, 1, 0,10	66,32,21,10
Nutation—8″, 4 Parallax 4″,0 - - - -	— 12,40	— 12,40
	28, 0,47,70	
Z. P. determined by 30 observations of Polaris above, and 30 below	38,31,21,15	
Z. P + ⊙ Z or N. P. D. - - - - -	66,32, 8,85	66 32, 8,70
Solsticial Declination - - - - - -	23,27,51,15	23,27,51,30
Correction for ⊙'s Latitude - - - - -	+, 0 95	+, 0,95
Mean Obliquity at Summer Solstice - - -	23,27,52,10	23,27,52,25

Subsequent observations gave the arc Z. P. rather greater than here assumed; but as I attribute this to a change in the refraction arising from a colder temperature, I take the arc as it was measured at the Summer Solstice.

This result may at any future time be rendered more accurate, if it should be found that the arc Z. P. has not been rightly determined; and in this consists the great advantage of the intermediate point Z., that the arc P. Z. may be determined more and more accurately, almost without limit, the only unalterable error resting on the determination of ⊙ Z.

IV. *Observations relative to the near and distant Sight of different Persons.* *By* James Ware, *Esq. F. R. S.*

Read November 19, 1812.

THE fact that near sightedness most commonly commences at an early period of life, and distant sightedness generally at an advanced age, is universally admitted. Exceptions, however, to these rules so frequently occur, that I flatter myself a brief statement of some of the coincident circumstances, attendant on these different imperfections in vision, may not be found wholly undeserving the attention of the Royal Society. Near sightedness usually comes on between the ages of ten and eighteen. The discovery of it most commonly arises from accident; and, at first, the inconvenience it occasions is so little, that it is not improbable the imperfection would remain altogether unnoticed, if a comparison were not instituted with the sight of others, or if the experiment were not made of looking through a concave glass. Among persons in the inferior stations of society, means are rarely resorted to for correcting slight defects of this nature; and, indeed, I have reason to believe the imperfection in such people is not unfrequently overcome by the increased exertions that are made by the eye to distinguish distant objects. This, however, is not the case, in the present day, with persons in the higher ranks of life. When these discover that their discernment of distant objects is less quick or less correct than that of others,

though the difference may be very slight, influenced perhaps by fashion more than by necessity, they immediately have recourse to a concave glass; the natural consequence of which is, that their eyes in a short time become so fixed in the state requiring its assistance, that the recovery of distant vision is rendered afterwards extremely difficult, if not quite impossible. With regard to the proportion between the number of near sighted persons in the different ranks of society, I have taken pains to obtain satisfactory information, by making inquiry in those places where a large number in these several classes are associated together. I have inquired, for instance, of the surgeons of the three regiments of foot guards, which consist of nearly ten thousand men; and the result has been, that near sightedness, among the privates, is almost utterly unknown. Not half a dozen men have been discharged, nor half a dozen recruits rejected, on account of this imperfection, in the space of nearly twenty years: and yet many parts of a soldier's duty require him to have a tolerably correct view of distant objects; as of the movements of the fugleman in exercise, and of the bull's eye when shooting at the target; the want of which might furnish a plausible apology for a skulker to skreen himself from duty, or to get his discharge from the service. I pursued my inquiries at the military school at Chelsea, where there are thirteen hundred children, and I found that the complaint of near sightedness had never been made among them until I mentioned it; and there were then only three who experienced the least inconvenience from it. After this, I inquired at several of the colleges in Oxford and Cambridge; and, though there is a great diversity in the number of students who make use of glasses in the various

colleges, they are used by a considerable proportion of the whole number in both Universities; and, in one college in Oxford, I have a list of the names of not less than thirty-two out of one hundred and twenty-seven, who wore either a hand glass or spectacles, between the years 1803 and 1807. It is not improbable, that some of these were induced to do it solely because the practice was fashionable; but, I believe, the number of such is inconsiderable, when compared with that of those whose sight received some small assistance from them, though this assistance could have been dispensed with, without inconvenience, if the practice had not been introduced. The misfortune resulting from the use of concave glasses is this, that the near sightedness is not only fixed by it, but a habit of inquiry is induced with regard to the extreme perfection of vision; and, in consequence of this, frequent changes are made for glasses that are more and more concave, until at length the near sightedness becomes so considerable, as to be rendered seriously inconvenient and afflicting. It should be remembered, that, for common purposes, every near sighted eye can see with nearly equal accuracy through two glasses, one of which is one number deeper than the other; and though the sight be in a slight degree more assisted by the deepest of these than by the other, yet on its being first used, the deepest number always occasions an uneasy sensation, as if the eye was strained. If, therefore, the glass that is most concave be at first employed, the eye, in a little time, will be accommodated to it, and then a glass one number deeper may be used with similar advantage to the sight; and if the wish for enjoying the most perfect vision be indulged, this glass may soon be changed for one that is a number still deeper, and so

in succession, until at length it will be difficult to obtain a glass sufficiently concave to afford the assistance that the eye requires.*

Although near sightedness is in general gradual in its progress, instances occasionally occur of its existence, in a considerable degree, even in children; in whom it is sometimes discovered almost as soon as they begin to take notice of the objects around them. This may be occasioned by some degree of opacity in the transparent parts of the eye; but such a cause of near sightedness is easily discovered by an examination, and is quite different from that state of the eye to which the term myopia, or near sightedness, is usually applied; by which is simply meant, too great a convexity either in the cornea or in the crystalline, in proportion to the distance of these parts from the retina. In such cases of extreme near sightedness in children, it is sometimes necessary to deviate from a rule, which in slighter cases I always follow, of discouraging the use of spectacles; since without their assistance, it would be impossible for them to prosecute their learning with ease or convenience.

Extreme near sightedness is sometimes occasioned by an evident change in the spherical figure of the cornea, and its assumption of a conical shape. This morbid state of the cornea is not only productive of near sightedness, but when the

* I have observed, that most of the near sighted persons, with whom I have had an opportunity of conversing, have had the right eye more near sighted than the left; and I think it not improbable, that this difference between the two eyes has been occasioned by the habit of using a single concave hand-glass; which, being most commonly applied to the right eye, contributes, agreeably to the remark abovementioned, to render this eye more near sighted than the other.

projection is considerable, vision is so much confused, that it affords little or no service, and cannot be amended by any glass. The cornea, in most of these cases, is preternaturally thin, and not unfrequently it is accompanied with symptoms of general debility; under which last circumstance chalybeate medicines, and bracing applications to the eye, have been found to afford considerable benefit.

Near sightedness, to an alarming degree, has sometimes attacked young persons suddenly. A remarkable case of this kind came under my notice a few years ago in a young gentleman at Westminster school, who had been attended by Sir GEORGE BAKER and Mr. SUTHERLAND, on account of a variety of anomalous nervous symptoms. These had wholly left him before I was consulted; and the consultation with me was solely for the purpose of determining whether he might be permitted to make use of concave glasses, and to return to the business of the school. The patient's health at that time not being perfectly restored, it was thought adviseable to send him for a few weeks into the country, and to postpone the use of glasses. This advice was followed; but in ten days the afflicted youth died suddenly. No anatomical examination of the head was permitted by the relatives. It seems, however, probable, that the near sightedness, as well as the previous indisposition, no less than the death of the patient, were occasioned by the pressure of a morbid substance of some kind or other on the source of the nerves in the brain.

Near sightedness is seldom alike in the two eyes, and a few cases have come under my observation, in which one eye of

the same person has had a near, and the other a distant sight.

It has been said by Dr. PORTERFIELD,* that the pupils of near sighted persons are more dilated than those of others. This, however, does not accord with the observations I have made in such cases.

It has also been commonly believed, that the size of the pupil is influenced by the distance of the object to which the attention is directed, this aperture being enlarged when the object is far off, and becoming more and more contracted as it is brought near. But though the activity of the fibres of the iris is sometimes sufficient to be visibly influenced by this circumstance, yet in the greater number even of those cases where the dilatation and contraction of the pupil are powerfully influenced by a difference in the strength of the light, the distance of the object considered alone, produces so little effect upon it, as to be scarcely perceived. That it has, however, in general, some degree of power on the pupil is highly probable; and an extraordinary instance of this kind exists, at the present time, in a lady between thirty and forty years of age, the pupil of whose right eye, when she is not engaged in reading, or in working with her needle, is always dilated very nearly to the rim of the cornea; but whenever she looks at a small object, nine inches from the eye, it contracts, within less than a minute, to a size nearly as small as the head of a pin. Her left pupil is not affected like the right; but in every degree of light and distance, it is contracted rather more than is usual in other persons. The vision is not precisely alike in the two eyes; the right eye being in a small degree near

* Treatise on the Eye and the Manner of Vision, Vol. II. p. 38.

sighted, and receiving assistance from the first number of a concave glass, whereas the left eye derives no benefit from it. This remarkable dilatation of the pupil of the right eye was first noticed about twenty years ago, and a variety of remedies have been employed at different times with a view to correct it; but none of them have made any alteration. It should be mentioned, that, in order to produce the contraction of the pupil, the object looked at must be placed exactly nine inches from the eye; and if it be brought nearer, it has no more power to produce the contraction than if it were placed at a remoter distance. It should also be mentioned, that the continuance of the contraction of the pupil depends, in some degree, on the state of the lady's health; since, though its contraction never remains long after the attention is withdrawn from a near object, yet whenever she is debilitated by a temporary ailment, the contraction is of much shorter duration than when her health is entire.*

Dr. WELLS, in his ingenious paper, published in the Second Part of the Transactions of the Royal Society for the year

* Several instances have come under my notice, in which the pupil of one eye has become dilated to a great degree, and has been incapable of contracting on an increase of light, whilst the pupil of the other eye has remained of its natural size. In some of these, the eye with the dilated pupil has been totally deprived of sight, the disorder answering to that of a perfect *amaurosis*; but in others, the dilatation of the pupil has only occasioned an inability to distinguish minute objects. Reading has been accomplished with difficulty, and convex glasses have afforded very little assistance. Though objects at a distance were seen with less inconvenience, than those that were near, these also appeared to the affected eye much less distinct than to the other. Most of the persons to whom I allude had been debilitated, by fatigue or anxiety, before the imperfection was discovered in the sight; and in some it had been preceded by affections of the stomach and alimentary canal.

1811, has taken pains to ascertain, whether the power by which the eye is adjusted to see at different distances, depends in any degree on the faculty in the pupil of dilating and contracting; and whether its fixed dilatation has any influence in preventing an accurate view of near objects. This last mentioned effect Dr. WELLS relates to have taken place remarkably in the case of Dr. CUTTING, whose pupil being fixed in a dilated state by the action of the extract of belladonna, perfect vision of a near object was removed, as the dilatation advanced, from six inches (which was the nearest distance at which Dr. CUTTING could distinctly see the image of the flame of a candle reflected from the bulb of a small thermometer,) to seven inches in thirty minutes, and to three feet and a half in three quarters of an hour. My eldest son, who has a very extensive range of vision, has made a similar experiment on his right eye with a similar result. Previous to the application of the belladonna, he could bring the apparent lines on an optometer (like that improved by Dr. YOUNG from the invention of Dr. PORTERFIELD, and described in the Philosophical Transactions for the year 1800) to meet at four inches from the eye; and, by directing his attention to a more distant point, he could prevent them from meeting till they were seven inches from the eye, after which they continued apparently united the whole length of the optometer, which was twelve inches.* He could see the image of a candle

* The two lines that are perceived on looking through the slits of an optometer, cross each other precisely in the point from whence the rays of light diverge in order to be brought to a focus on the retina. And their apparent union before and after this point is occasioned by the unavoidable thickness of the line drawn on the optometer.

reflected from the bulb of a small thermometer, five-sixteenths of an inch in diameter, at the distance of three inches and three quarters from the eye; and he could also see the same image at the distance of two feet seven inches. The belladonna produced a conspicuous dilatation of the pupil in less than an hour; after which, on viewing the apparent lines on the optometer, he was unable to make them meet at a nearer distance than seven inches, or to gain a distinct image of the candle reflected by the bulb of the thermometer nearer than this distance; but he could discern it at two feet ten inches from the eye, which was three inches further than he was able to see it, before the belladonna was applied. During the time of the experiment on the right eye, the left eye possessed its usual range of vision, but the sight, when both eyes were open, was rather confused, in consequence of the unequal foci of the two eyes; and it did not become clear until the pupil of the right eye recovered its usual power of contracting, which power was not acquired till the third day after the application of the belladonna.

It is remarkable that a different effect is sometimes produced on a near sighted eye by the application of the belladonna, from that which it has on an eye that enjoys a distant sight. Dr. WELLS made an experiment of this kind on a friend of his, who was near sighted; and he informs us, in the paper above referred to, that in this instance, the nearest point of perfect vision was moved forwards during the dilatation of the pupil, whilst its remote point remained unaltered. I have made a similar experiment on the eyes of several such persons; and though in two of these the result appeared to be similar to that which has been mentioned by Dr. WELLS, yet,

in the greater number, their sight, like that of those who were not myopic, has become more distant as the pupil became more dilated.—In one gentleman, in whom the lines of the optometer appeared to meet at four inches and a quarter from the eye, the pupil, in half an hour after the application of the belladonna, became completely dilated, and in consequence of this the sight at first was confused; but both on that day, and for two days afterwards, it was evidently more distant, and the apparent lines on the optometer could not be made to meet nearer than seven inches from the eye.—In a young lady, seventeen years of age, whose right eye was so near sighted that the apparent lines on the optometer met at two inches and three quarters from the eye, these lines, when the pupil was dilated (which took place in a small degree in less than half an hour), could not be made to meet in less than three inches and a quarter; and on the following day, the pupil being more dilated, the lines did not meet till they were at the distance of nearly four inches.—In a third instance, viz. that of a lady forty-five years of age, who had been remarkably near sighted from her infancy, and for many years had used concave glasses of the fifteenth number, (which number is ground on each side, upon a tool the radius of which is only three inches,) the sight was become so confused in both eyes, that she saw nothing distinctly, and was unable to read letters, of the size that are used in the printed Transactions of the Royal Society, either with or without a glass. In this case, after the pupils had been dilated by the application of the belladonna, the sight was so much improved that she was able to read a print of the abovementioned size at the distance

of two inches with either eye. I do not insist, however, on the present case, because, though there was not any visible opacity in the crystalline, this sometimes exists in a small degree without being perceptible even to an attentive observer; and it may be doubted whether the amendment in the lady's vision, were not occasioned solely by the retraction of the iris from before a part of the crystalline that was not yet become opaque: it being well known that the outer part of this lens not unfrequently retains its transparency for some time after an opacity has commenced in the part that surrounds its centre.

It is evident, that near sightedness has no dependence on the greater or smaller degree of convexity possessed by the cornea, when this circumstance is considered alone; since the length of the axis of the eye from the cornea to the retina, and the greater or smaller degree of convexity in the crystalline humour, must be also regarded, before the distance of accurate vision can be determined.

It is no less evident, that near sightedness is not necessarily occasioned by a morbid protrusion of the whole eye; since some persons are born with eyes of this description, and others acquire the peculiarity, when further advanced in life, in consequence of a morbid accumulation of *adeps* at the bottom of the orbit, without either of them being more near sighted than those who are free from this imperfection.

I have seen many instances in which old persons, who have been long accustomed to use convex glasses of considerable power, have recovered their former sight at the advanced age of eighty or ninety years, and have then had no further need of them. Dr. PORTERFIELD was of opinion that in such cases

the amendment is occasioned by a decay of *adeps* at the bottom of the orbit; in consequence of which the eye, from a want of the usual support behind, is brought, by the pressure of the muscles on its sides, into a kind of oval figure, in which state the retina is removed to its due focal distance from the flattened cornea. But if a morbid absorption of *adeps* at the bottom of the orbit were sufficient to restore the presbyopic to a good sight, it might be expected, that a morbid accumulation of *adeps* in this part would produce a presbyopic or distant sight. This, however, has not happened in any of the cases that have come under my notice. On the contrary, in some such persons a degree of near sightedness has been induced by the accumulation; and in others the sight, with regard to distance, has not been affected by it. It appears to me more probable, that this remarkable revolution in the sight of old persons is occasioned by an absorption of part of the vitreous humour; in consequence of which, the sides of the sclerotica are pressed inward, and the axis of the eye, by this lateral pressure, is proportionably lengthened. An alteration of this kind is also sufficient to explain the reason, why such aged persons retain the power of distinguishing objects at a distance, at the same time that they recover the faculty of seeing those that are near; since the lengthened axis of the eye leaves the power by which it is adjusted to see at different distances, precisely in the same state, in which it was before the lengthening of the axis took place.*

* Dr. YOUNG, in the paper to which I alluded in page 38, has described a great number of ingenious experiments devised by him, to shew that the faculty of seeing at different distances is produced by a power in the crystalline humour, to become more or less convex, according as the object is more or less distant from the eye.

Although old persons lose the power of distinguishing correctly near objects, and require for this purpose the aid of convex glasses, they usually retain the sight of those that are distant as well as when they were young. Instances, however, are not wanting of persons advanced in life, who require the aid of convex glasses to enable them to see near, as well as distant, objects. Dr. WELLS is one of these. He informs us, in the paper to which I have more than once adverted, that when twenty years younger, he was able, with his left eye, to bring to a focus on the retina, pencils of rays which flowed from every distance greater than seven inches from the cornea; but at the age of fifty-five, he required not only a convex glass of six inches focus, to enable him to bring to a point on the retina rays proceeding from an object seven inches from the eye, but likewise a convex glass of thirty-six inches focus, to enable him to bring to a point parallel rays.—There are also instances of young persons, who have so disproportionate a convexity of the cornea or crystalline, or of both, to the distance of these parts from the retina, that a glass of considerable convexity is required to enable them to see distinctly, not only near objects, but also those that are distant; and it is remarkable, that the same glass will enable many such persons to see both near and distant objects; thus proving that the defect in their sight is occasioned solely by too small a convexity in one of the parts abovementioned, and that it does not influence the power by which their eyes are adapted to see at distances variously remote. In this respect such persons differ from those who have had the crystalline humour removed by an operation; since the latter always require a glass to enable them to discern distant objects, different from

that which they use to see those that are near. This circumstance, in my apprehension, affords a convincing proof that the crystalline humour is indispensably necessary to enable the eye to see at different distances.—It is also worthy of remark, that persons who have had the crystalline humour removed, have less power to ascertain the distance of an object when they look through a convex glass, than when they view it without this assistance; in consequence of which such persons seldom make use of glasses when they are walking: and the inconvenience of glasses is particularly experienced when they descend a flight of steps, or pass over uneven ground.

Near sighted persons do not appear to possess the same extent of vision that is enjoyed by those who have a distant sight. Being near sighted, I have repeatedly endeavoured to ascertain my own range of vision: and I find, by examining the focus of my right eye through the abovementioned optometer, that I see two converging lines, which appear to meet, with very slight variations, at the distance of three inches from the eye; and no effort I am able to make can keep these lines united further than the distance of four inches and a quarter. They then separate, and continue to diverge. With my left eye, the lines do not appear to meet nearer than four inches, and they continue united as far as five inches and a quarter, after which they also separate and diverge; so that the range of distinct vision in me does not extend further than an inch and a quarter in either eye; and within these distances I always hold a book when I read.—I find also the following rule, for determining the concavity of the glass that is best adapted for near sighted persons, to be perfectly correct with respect to myself, and, I believe, it may be safely

adopted by those who, from distance or any other cause, are unable to suit themselves at the shop of an expert optician. The rule is this. Multiply the distance at which the person reads with ease, (which, with my left or best eye, is five inches,) by that at which he wishes to read, which may be said to be twelve inches; divide the product, sixty, by seven, the difference between the two, and it leaves nearly nine inches for the focus of the concave glass that shall produce the desired effect. This is the exact concavity of the glass that I am obliged to use, to enable me to read with ease; and it answers to that, sold under the name of No. *6*; which, I am informed by Mr. Blunt the optician, is a double concave glass, ground on a tool of eight inches radius on one side, and eleven inches on the other, the mean between which is very nearly nine inches. With a glass of this description I can read the smallest print, but to distinguish distant objects I am obliged to look through that, denominated No. 9, by opticians, which is ground on a tool of nine inches radius on both sides. In this respect, my eye has varied from what it was a few years ago, when I was able to distinguish both near and distant objects correctly, through No. 8. This is ground to a radius of eight inches on one side, and six inches on the other, and with it I can still read a type like that in which the Transactions of the Royal Society are printed; but am unable to distinguish through it many distant objects, which I formerly used to see distinctly. —Hence it appears that my eyes have a confined range of distinct vision, extending only to an inch, or an inch and a quarter; and that they remain nearly in the same state in which they were many years ago with regard to near objects, but have lost a part of the power which they formerly

possessed, of adjusting themselves to distant ones. In this last respect, they differ from the eyes of those who have naturally a distant sight, since, as such persons advance in life, they usually retain the power of distinguishing distant objects, but lose that of seeing those that are near. It appears to militate also against the common observation, that as near sighted persons grow older they become less near sighted; since my eyes, on the contrary, are more near sighted, at the age of fifty-five, than they were at twenty-five, and I am now obliged to employ deeper concave glasses than I then used to see distant objects, though I am not able to see distinctly through them things that are near.

The alteration which has taken place in my range of vision, I have reason to believe, is not unusual. Dr. WELLS, in his paper on this subject, mentions the case of a gentleman, who, like me, was near sighted, and whose sight, as he advanced in life, had undergone a similar change.—The following is also an instance of this kind, that is still more remarkable. Mr. L. sixty-six years of age, who has spent a great part of his life in the West Indies, and whose sight, when he was young, enabled him to see both near and distant objects with great precision, began, at the age of forty, to experience a difficulty in reading and writing. He immediately procured convex spectacles of the first number sold by opticians, which glasses are usually ground to a focus of forty-six or forty-eight inches, and by the aid of these he continued to read and write with ease (distinguishing perfectly in the usual way all distant objects without them,) until he was fifty. At this time he first began to perceive an indistinctness in the appearance of things at a distance; and, on

trying with different glasses, he discovered that, by looking through a double concave glass of the sixth number, (which is ground to a radius of eight inches on one side and eleven inches on the other,) he was enabled to see distant objects distinctly. He has continued to use glasses of this description for the purpose of seeing distant objects from that time to the present; but is obliged to remove them whenever he reads, and still to employ the first number of a convex glass.—In this instance, a presbyopic was changed to a myopic sight, without any known efficient circumstance to produce it.—In the two following cases, a similar change took place; and in them it was attributable to known causes. A woman, about fifty years of age, of a full habit, who for several years had been obliged to make use of convex glasses, in order to read a small print, was seized with a dimness in the sight of the right eye, accompanied with a small degree of inflammation. The sight of the left eye having been long imperfect, this affection of the right eye occasioned a great depression of spirits. Recourse was necessarily had to copious evacuations, by means of which the inflammation and dimness of sight were soon removed; but afterwards the patient was much alarmed on finding that the spectacles she had been accustomed to wear, instead of affording their usual assistance, confused her sight. Upon this discovery, she was induced to look through her husband's glasses, which, in consequence of his being near sighted, were double concaves of the fifth number, and ground to a radius of eleven inches on each side. These did not assist her in looking at near objects, but by their aid she saw much more distinctly those that were distant; and, on attempting to read, nothing more was now necessary, than to bring

the book a little nearer to her, than she had been previously accustomed to place it.—The second case occurred in a patient about the same age, who, in the course of the last year, was attacked with an inflammation in both eyes. By the use of leaches and cooling medicines, it was speedily removed, and, afterwards, she was much gratified, by finding that the necessity for using glasses when she read, which had existed many years, was removed; and that she could see both near and distant objects correctly, without any extraneous help. The amendment in this lady's sight continued, however, only a few weeks; after which she was again obliged to use the same convex glasses in looking at small near objects, which she had used before her eyes became inflamed.—In addition to these cases, I beg leave to add the information I have received from an eminent mathematical instrument maker, about fifty years of age, who has long made use of convex glasses to assist his sight in reading. He tells me, that when he has been employed many hours together, for several successive days, in looking through a double microscope that magnifies twenty-eight times, (in order to enable him to mark the degrees on a small brass plate) he has afterwards been able, repeatedly, for a few weeks, to read without his glasses; but then the amendment gradually ceases, and he is soon obliged to return to the use of the same glasses that he had worn before.

In the instances that have been mentioned, the distant sightedness affected persons who were considerably advanced in life: but in the three that follow, a similar affection of the sight occurred in those that were young; and a like good effect was produced by the use of evacuating remedies. One

of these was a boy eight years old, who suddenly became presbyopic, and had repeatedly been punished at school, on account of his incorrect and defaced writing; the real cause of it, at that time, being unknown to his master. After the presbyopia had continued a fortnight, and different local applications had been used, without producing any sensibly good effects, the lad was cured by the application of leaches to the temples, and the administration of a few purgative medicines. The other instances occurred in two daughters of the same family. The eldest, twenty years of age, had never been able to do fine work, and for three years had been greatly assisted by convex spectacles. The youngest, a girl of fifteen, had become presbyopic about a year ago, and since that time had been obliged to use spectacles whenever she read, or worked with her needle. The young person, last mentioned, in the course of six weeks, (during which time she totally abstained from the use of glasses,) was completely relieved from the necessity of using them, by the application of two leaches to each temple twice in a week. The former, in the same space of time, experienced much relief from a similar treatment, but was still unable to do fine work without glasses, partly in consequence of the long continuance of the infirmity, and partly on account of her not having abstained with equal steadiness from the occasional use of them.

From the preceding statement, the following inferences may be deduced.

First; near sightedness is rarely observed in infants, or even in children under ten years of age. It affects the higher classes of society more than the lower: and the instances are few, if any, in which, if the use of concave glasses has been

adopted, increasing years have either removed or lessened this imperfection.

Secondly; though the usual effect of time on perfect eyes be that of inducing a necessity to make use of concave glasses, in order to see near objects distinctly, yet sometimes, even after the age of fifty, and after convex glasses have been used many years for this purpose, the eyes have not only ceased to derive benefit from them, when looking at near objects, but they have required concave glasses to enable them to distinguish, with precision, objects at a distance.

Thirdly; though the cause of this change be not always known, yet sometimes it has been induced by the use of evacuating remedies, particularly of leaches applied to the temples; and sometimes by looking through a microscope, for a continued length of time, in several successive days.

Fourthly; instances are not uncommon, in which persons, far advanced in life, (viz. between eighty and ninety,) whose eyes have been accustomed for a long time to the use of deeply convex glasses, when they have read or written, have ceased to derive benefit from these glasses, and they have become able, without any assistance, to see both near and distant objects almost as well as when they were young. Although it be not easy to ascertain the cause of this amended vision, it seems not improbable that it is occasioned by an absorption of part of the vitreous humour; in consequence of which the sides of the eye collapse, and its axis, from the cornea to the retina, is lengthened; by which alteration the length of this axis is brought into the same proportion to the flattened state of the cornea or crystalline, or both, which it had to these parts before the alteration took place.

V. *The Bakerian Lecture. On the elementary Particles of certain Crystals. By* William Hyde Wollaston, *M. D. Sec. R. S.*

Read November 26, 1812.

Among the known forms of crystallized bodies, there is no one common to a greater number of substances than the regular octohedron, and no one in which a corresponding difficulty has occurred with regard to determining which modification of its form is to be considered as primitive; since in all these substances the tetrahedron appears to have equal claim to be received as the original from which all their other modifications are to be derived.

The relations of these solids to each other is most distinctly exhibited to those who are not much conversant with crystallography, by assuming the tetrahedron as primitive, for this may immediately be converted into an octohedron by the removal of four smaller tetrahedrons from its solid angles. (Fig. 1.)

The substance which most readily admits of division by fracture into these forms is fluor spar; and there is no difficulty in obtaining a sufficient quantity for such experiments. But it is not, in fact, either the tetrahedron or the octohedron, which first presents itself as the apparent primitive form obtained by fracture.

If we form a plate of uniform thickness by two successive divisions of the spar, parallel to each other, we shall find the

plate divisible into prismatic rods, the section of which is a rhomb of 70° 32′ and 109° 28′ nearly; and if we again split these rods transversely, we shall obtain a number of regular acute rhomboids, all similar to each other, having their superficial angles 60° and 120°, and presenting an appearance of primitive molecule, from which all the other modifications of such crystals might very simply be derived. And we find, moreover, that the whole mass of fluor might be divided into, and conceived to consist of, these acute rhomboids alone, which may be put together so as to fit each other without any intervening vacuity.

But, since the solid thus obtained (as represented fig. 2.) may be again split by natural fractures at right angles to its axis (fig. 3.), so that a regular tetrahedron may be detached from each extremity, while the remaining portion assumes the form of a regular octohedron; and, since every rhomboid, that can be obtained, must admit of the same division into one octohedron and two tetrahedrons, the rhomboid can no longer be regarded as the primitive form; and since the parts into which it is divisible are dissimilar, we are left in doubt which of them is to have precedence as primitive.

In the examination of this question, whether we adopt the octohedron or the tetrahedron as the primitive form, since neither of them can fill space without leaving vacuities, there is a difficulty in conceiving any arrangement in which the particles will remain at rest: for, whether we suppose, with the Abbé HAÜY, that the particles are tetrahedral with octohedral cavities, or, on the contrary, octohedral particles regularly arranged with tetrahedral cavities, in each case the mutual contact of adjacent particles is only at their edges; and

although in such an arrangement it must be admitted that there may be an equilibrium, it is evidently unstable, and ill adapted to form the basis of any permanent crystal.

More than three years have now elapsed since a very simple explanation of this difficulty occurred to me. As in the course of that time I had not discovered it to be liable to any crystallographical objection, and as it had appeared satisfactory to various mathematical and philosophical friends to whom I proposed it, I had engaged to make this the subject of the Bakerian Lecture of the present year, hoping that some further speculations, connected with the same theory, might lead to more correct notions than are at present entertained of crystallization in general.

At the time when I made this engagement, I flattered myself that the conception might be deserving of attention from its novelty. But I have since found, that it is not altogether so new as I had then supposed it to be; for by the kindness of a friend, I have been referred to Dr. HOOKE's Micrographia, in which is contained, most clearly, one essential part of the same theory.

However, since the office of a lecturer is properly to diffuse knowledge already acquired, rather than to make known new discoveries in science, and since these hints of Dr. HOOKE have been totally overlooked, from having been thrown out at a time when crystallography, as a branch of science, was wholly unknown, and consequently not applied by him to the extent which they may now admit, I have no hesitation in treating the subject as I had before designed. And when I have so done, I shall quote the passage from Dr. HOOKE, to shew how exactly the views which I have taken have, to a certain extent,

corresponded with his; and I shall hope that, by the assistance of such authority, they may meet with a more favourable reception.

The theory to which I here allude is this, that, with respect to fluor spar and such other substances as assume the octohedral and tetrahedral forms, all difficulty is removed by supposing the elementary particles to be perfect spheres, which by mutual attraction have assumed that arrangement which brings them as near to each other as possible.

The relative position of any number of equal balls in the same plane, when gently pressed together, forming equilateral triangles with each other (as represented perspectively in fig. 4.) is familiar to every one; and it is evident that, if balls so placed were cemented together, and the stratum thus formed were afterwards broken, the straight lines in which they would be disposed to separate would form angles of 60° with each other.

If a single ball were placed any where at rest upon the preceding stratum, it is evident that it would be in contact with three of the lower balls (as in fig. 5.), and that the lines joining the centres of four balls so in contact, or the planes touching their surfaces, would include a regular tetrahedron, having all its sides equilateral triangles.

The construction of an octohedron, by means of spheres alone, is as simple as that of the tetrahedron. For if four balls be placed in contact on the same plane in form of a square, then a single ball resting upon them in the centre, being in contact with each pair of balls, will present a triangular face rising from each side of the square, and the whole together will represent the superior apex of an octohedron; so that a

sixth ball similarly placed underneath the square will complete the octohedral group, fig. 6.

There is one observation with regard to these forms that will appear paradoxical, namely, that a structure which in this case was begun upon a square foundation, is really intrinsically the same as that which is begun upon the triangular basis. But if we lay the octohedral group, which consists of six balls, on one of its triangular sides, and consequently with an opposite triangular face uppermost, the two groups, consisting of three balls each, are then situated precisely as they would be found in two adjacent strata of the triangular arrangement. Hence in this position we may readily convert the octohedron into a regular tetrahedron, by addition of four more balls. (fig. 7.) One placed on the top of the three that are uppermost forms the apex; and if the triangular base, on which it rests, be enlarged by addition of three more balls regularly disposed around it, the entire group of ten balls will then be found to represent a regular tetrahedron.

For the purpose of representing the acute rhomboid, two balls must be applied at opposite sides of the smallest octohedral group, as in fig. 9. And if a greater number of balls be placed together, fig. 10 and 11, in the same form, then a complete tetrahedral group may be removed from each extremity, leaving a central octohedron, as may be seen in fig. 11, which corresponds to fig. 3.

The passage of Dr. Hooke, from which I shall quote so much as to connect the sense, is to be found at page 85 of his Micrographia.

" From this I shall proceed to a second considerable phe-
" nomenon, which these diamants (meaning thereby quartz

"crystals) exhibit, and that is the regularity of their figure "——This I take to proceed from the most simple principle "that any kind of form can come from, next the globular; "for——I think I could make probable, that all these regular "figures arise only from three or four several positions or "postures of globular particles, and those the most plain and "obvious, and necessary conjunctions of such figured particles "that are possible——And this I have *ad oculum* demonstrated "with a company of bullets, so that there was not any regu-"lar figure which I have hitherto met withal of any of those "bodies that I have above named, that I could not with the "composition of bullets or globules imitate almost by shaking "them together.

"Thus, for instance, we find that globular bullets will of "themselves, if put on an inclining plain so that they may "run together, naturally run into a triangular order compos-"ing all the variety of figures that can be imagined out of "equilateral triangles, and such you will find upon trial all the "surfaces of alum to be composed of——

"—nor does it hold only in superficies, but in solidity also; "for it's obvious that a fourth globule laid upon the third in "this texture composes a regular tetrahedron, which is a very "usual figure of the crystals of alum. And there is no one "figure into which alum is observed to be crystallized, but "may by this texture of globules be imitated, and by no "other."

It does not appear in what manner this most ingenious philosopher thought of applying this doctrine to the formation of quartz crystal, of vitriol, of salt-petre, &c. which he names. This remains among the many hints which the peculiar jealousy

of his temper left unintelligible at the time they were written, and which, notwithstanding his indefatigable industry, were subsequently lost to the public, for want of being fully developed.

We have seen, that by due application of spheres to each other, all the most simple forms of one species of crystal will be produced, and it is needless to pursue any other modifications of the same form, which must result from a series of decrements produced according to known laws.

Since then the simplest arrangement of the most simple solid that can be imagined, affords so complete a solution of one of the most difficult questions in crystallography, we are naturally led to inquire what forms would probably occur from the union of other solids most nearly allied to the sphere. And it will appear that by the supposition of elementary particles that are spheroidical, we may frame conjectures as to the origin of other angular solids well known to crystallographers.

The obtuse Rhomboid.

If we suppose the axis of our elementary spheroid to be its shortest dimension, a class of solids will be formed which are numerous in crystallography. It has been remarked above, that by the natural grouping of spherical particles, fig. 10, one resulting solid is an acute rhomboid, similar to that of fig. 2, having certain determinate angles, and its greatest dimension in the direction of its axis. Now, if other particles having the same relative arrangement be supposed to have the form of oblate spheroids, the resulting solid, fig. 12, will still be a regular rhomboid; but the measures of its angles will be different from those of the former, and will be more

or less obtuse according to the degree of oblateness of the primitive spheroid.

It is at least possible that carbonate of lime and other substances, of which the forms are derived from regular rhomboids as their primitive form, may, in fact, consist of oblate spheroids as elementary particles.

It deserves to be remarked, that the conjecture to which we are thus led by a natural transition, from consideration of the most simple form of crystals, was long since entertained by HUYGHENS,* when treating of the oblique refraction of Iceland spar, which he so skilfully analysed. The peculiar law observable in the refraction of light by that crystal, he found might be explained on the supposition of spheroidical undulations propagated through the substance of the spar, and these he thought might perhaps be owing to a spheroidical form of its particles, to which the disposition to split into the rhomboidal form might also be ascribed.

By some oversight, however, the proportion of the axes of such an elementary spheroid is erroneously stated to be 1 to 8; but this is probably an error of the press, instead of 1 to 2,8, for I find the proportion to be nearly 1 to 2,87. In fig. 15, F is the apex of a tetrahedron cut from an acute rhomboid similar to fluor spar, and the sections of two spheres are represented round the centres F and C. I is the apex of a corresponding portion cut from the summit of a rhomboid of Iceland spar, as composed of spheroids having the same diameter as the spheres. In the former, the inclination FCT of the edge of the tetrahedron to its base is 54° 44′; in the latter, the inclination ICT is 26° 15′; and the altitudes FT, IT are as

* HUYGHENII Op. Reliq. Tom. I. Tract. de Lumine, p 70.

the tangents of these angles 1414 to 493 : : 2,87 : 1, which also expresses the ratio of the axis of the sphere to that of the spheroid, or the proportional diameters of the generating ellipse.

Hexagonal Prisms.

If our elementary spheroid be on the contrary oblong, instead of oblate, it is evident that by mutual attraction, their centres will approach nearest to each other when their axes are parallel, and their shortest diameters in the same plane (fig. 13.) The manifest consequence of this structure would be, that a solid so formed would be liable to split into plates at right angles to the axes, and the plates would divide into prisms of three or six sides with all their angles equal, as occurs in phosphate of lime, beryl, &c.

It may further be observed, that the proportion of the height to the base of such a prism must depend on the ratio between the axes of the elementary spheroid.

The Cube.

Although I could not expect that the sole supposition of spherical or spheroidical particles would explain the origin of all the forms observable among the more complicated crystals, still the hypothesis would have appeared defective, if it did not include some view of the mode in which so simple a form as the cube may originate.

A cube may evidently be put together of spherical particles arranged four and four above each other, but we have already seen that this is not the form which simple spheres are naturally disposed to assume, and consequently this hypothesis

alone is not adequate to its explanation, as Dr. HOOKE had conceived.

Another obvious supposition is that the cube might be considered as a right angled rhomboid, resulting from the union of eight spheroids having a certain degree of oblateness (2 to 1) from which a rectangular form might be derived. But the cube so formed would not have the properties of the crystallographical cube. It is obvious, that, though all its diagonals would thus be equal, yet one axis parallel to that of the elementary spheroid would probably have properties different from the rest. The modifications of its crystalline form would probably not be alike in all directions as in the usual modifications of the cube, but would be liable to elongation in the direction of its original axis. And if such a crystal were electric, it would have but one pair of poles instead of having four pair, as in the crystals of boracite.

There is, however, an hypothesis which at least has simplicity to recommend it, and if it be not a just representation of the fact, it must be allowed to bear a happy resemblance to truth.

Let a mass of matter be supposed to consist of spherical particles all of the same size, but of two different kinds in equal numbers, represented by black and white balls; and let it be required that in their perfect intermixture every black ball shall be equally distant from all surrounding white balls, and that all adjacent balls of the same denomination shall also be equidistant from each other. I say then, that these conditions will be fulfilled, if the arrangement be cubical, and that the particles will be in equilibrio. Fig. 14 represents a cube so constituted of balls, alternately black and

white throughout. The four black balls are all in view. The distances of their centres being every way a superficial diagonal of the cube, they are equidistant, and their configuration represents a regular tetrahedron; and the same is the relative situation of the four white balls. The distances of dissimilar adjacent balls are likewise evidently equal; so that the conditions of their union are complete, as far as appears in the small group: and this is a correct representative of the entire mass, that would be composed of equal and similar cubes.

Since the crystalline form and electric qualities of boracite are perhaps unique, any explanation of properties so peculiar can hardly be expected. It may, however, be remarked, that a possible origin of its four pair of poles may be traced in the structure here represented; for it will be seen that a white ball and a black one are regularly opposed to each other at the extremities of each axis of the cube.

An hypothesis of uniform intermixture of particle with particle, accords so well with the most recent views of binary combination in chemistry, that there can be no necessity, on the present occasion, to enter into any defence of that doctrine, as applied to this subject. And though the existence of ultimate physical atoms absolutely indivisible may require demonstration, their existence is by no means necessary to any hypothesis here advanced, which requires merely mathematical points endued with powers of attraction and repulsion equally on all sides, so that their extent is *virtually* spherical, for from the union of such particles the same solids will result as from the combination of spheres impenetrably hard.

There remains one observation with regard to the spherical form of elementary particles, whether actual or virtual, that

must be regarded as favourable to the foregoing hypothesis, namely, that many of those substances, which we have most reason to think simple bodies, as among the class of metals, exhibit this further evidence of their simple nature, that they crystallize in the octohedral form, as they would do if their particles were spherical.

But it must, on the contrary, be acknowledged, that we can at present assign no reason why the same appearance of simplicity should take place in fluor spar, which is presumed to contain at least two elements; and it is evident that any attempts to trace a general correspondence between the crystallographical and supposed chemical elements of bodies must, in the present state of these sciences, be premature.

Note. A theory has lately been advanced * by M. PRECHTL, which attempts to account for various crystalline forms from the different degrees of compression that soft spheres may be supposed to undergo in assuming the solid state. It is supposed, that with a certain degree of softness and of relative attraction, the particles will be surrounded each by four others, and will all be tetrahedral, although in fact it be demonstrably impossible that tetrahedrons alone should fill any space.

It is next supposed, that soft spheres less compressed will be surrounded by five others, and will be formed into triangular prisms, comprised under five similar and equal planes. That they should be similar is impossible, and it is further demonstrable, that when the triangular termination of such a

* Journal des Mines, No. 166.

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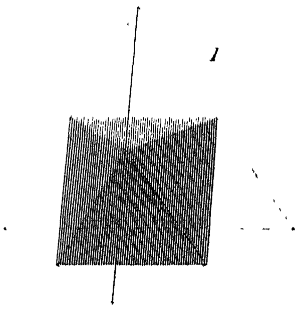

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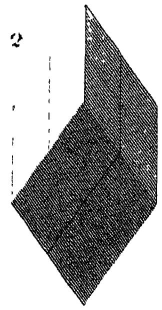

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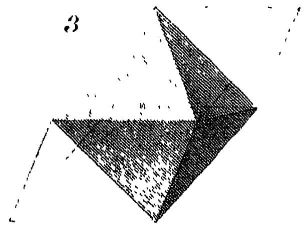

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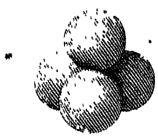

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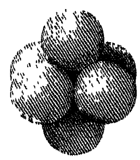

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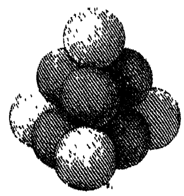

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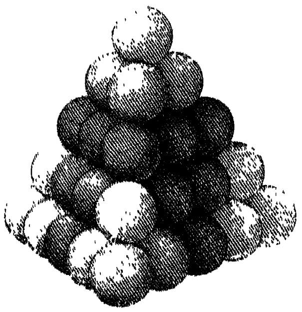

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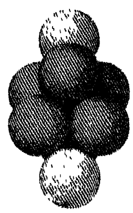

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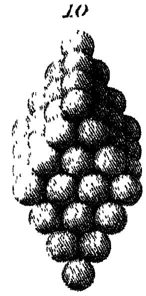

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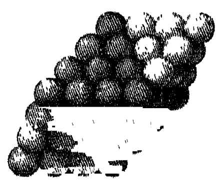

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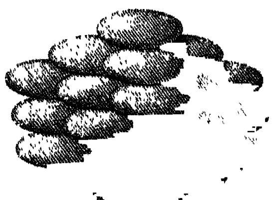

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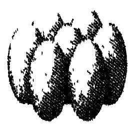

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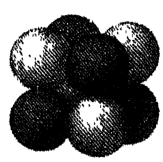

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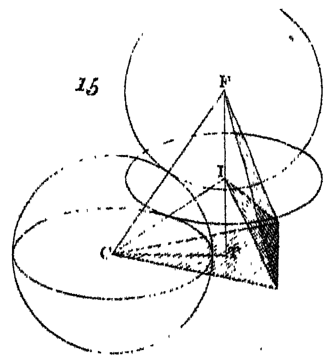


Engraved by W. Lowry.

prism is equal in area to each rectangular side of the prism, so as to present equal resistance, according to the hypothesis, then the triangular faces will be nearer to the centre in the proportion of three to four, so that the attractions will not be equal as the hypothesis would require.

A third hypothesis of M. Prechtl is, that the degree of compressibility may be such that each particle will be surrounded by six others, giving it the form of a cube, which, it must be admitted, is a very possible supposition.

All further application of the same hypothesis is precluded by M. Prechtl, by denying that one particle can be surrounded by more than six others; although in fact it is most evident, that any sphere when not compressed will be surrounded by twice that number, and consequently by a slight degree of compression will be converted into a dodecahedron, according to the most probable hypothesis of simple compression.

VI. *On a Substance from the Elm Tree, called Ulmin.* *By* James Smithson, *Esq. F. R. S.*

Read December 10, 1812.

1. THE substance now denominated Ulmin was first made known by the celebrated Mr. KLAPROTH, to whom nearly every department of chemistry is under numerous and great obligations.*

Ulmin has been ranked by Dr. THOMSON, in his System of Chemistry, as a distinct vegetable principle, on the ground of its possessing qualities totally peculiar and extraordinary. It is said, that though in its original state easily soluble in water and wholly insoluble in alcohol and ether, it changes, when nitric, or oxymuriatic acid is poured into its solution, into a resinous substance no longer soluble in water, but soluble in alcohol, and this singular alteration is attributed to the union to it of a small portion of oxygen which it has acquired from these acids.* Being possessed of some of this substance which had been sent to me some years ago from Palermo, by the same person from whom Mr. KLAPROTH had received it, I became induced, by the foregoing account, to pay attention to it, and have observed facts which appear to warrant a different etiology of its phenomena, and opinion of its nature, from what has been given of them.

The ulmin made use of in the following experiments, had

* Dr. THOMSON's Syst. of Chem. Vol. IV. p. 696. Fourth edition.

been freed from the fragments of bark by solution in water and filtration, and recovered in a dry state by the evaporation of the solution on a water bath.

2. In lumps, ulmin appears black, but in thin pieces it is seen to be transparent, and of a deep red colour.

In a dilute state, solution of ulmin is yellow; in a concentrated one, dark red, and not unlike blood.

When solution of ulmin dries, either spontaneously or by being heated, the ulmin divides into long narrow strips disposed in rays to the centre, which curl up and detach themselves from the vessel, and the fluid part seems to draw together, and becomes remarkably protuberant. Solution of ulmin slowly and feebly restores the colour of turnsol paper reddened by an acid.

3. Dilute nitric acid being poured into a solution of ulmin, a copious precipitate immediately formed. The mixture was thrown on a filter. The matter which has been considered as a resin remained on the paper, and a clear yellow liquor came through. This yellow solution, on evaporation, produced a number of prismatic crystals looking like nitrate of potash. They were tinged yellow by some of the resin. This mixture, heated in a gold dish, deflagrated with violence, and a large quantity of fixed alkali remained.

Dilute muriatic acid caused an exactly similar precipitation in solution of ulmin to nitric acid, and the precipitate was the same resin-like substance. The filtered liquor afforded a quantity of saline matter, which, after being freed by ignition from a portion of dissolved resin, shot into pure white cubes of muriate of potash, as appeared by decomposing them by nitric acid.

Sulphuric, phosphoric, oxalic, tartaric, and citric acids, occasioned a similar precipitation in solution of ulmin.

Distilled vinegar produced no turbidness in it; and the mixture being exhaled to dryness, at a gentle heat, was found to be again wholly soluble in water. But when the mixture was made to boil, some decomposition took place. On adding muriatic acid to a mixture of solution of ulmin and distilled vinegar, a precipitate was produced, as in a mere solution in water.

The nitric and muriatic acids received a small quantity of lime and iron from the ulmin, and I believe also a little magnesia; but these can be considered only as foreign admixtures.

4. To acquire an idea of the quantity of potash in ulmin, 4 grains of ulmin were decomposed by nitric acid. They afforded 2.4 grains of resin-like matter. The nitrate of potash obtained was heated to deflagration, in small quantities at a time, in a platina crucible to free it from resin. The alkali produced was supersaturated with nitric acid, dried, and slightly fused. It then weighed 1.2 grains. If we admit $\frac{1}{2}$ of nitrate of potash to be alkali, this will denote $\frac{15}{100}$ of potash in ulmin.

5 grains of ulmin were decomposed by muriatic acid. The resinous matter weighed 3.3 grains, and the muriate of potash, after being ignited, dissolved away from the charcoal, dried, and again made red hot, weighed 1.4 grains. If we suppose $\frac{2}{3}$ of muriate of potash to be alkali, this will indicate $\frac{19}{100}$ of potash in ulmin.

2 grains of ulmin were made red hot in a gold crucible. It then weighed only 1.05 grain. The form of the flakes was

in no degree altered, but they had acquired the blue and yellow colours of heated steel, of which they had likewise the metallic aspect and lustre, and could difficultly, if at all, have been distinguished by the eye from heated steel-filings, or fragments of slender watch-springs. Water immediately destroyed their metallic appearance.

Muriatic acid, poured on, caused a strong effervescence, and formed muriate of potash, which, freed from all charcoal, and made red hot, weighed 0.6 grain, corresponding to $\frac{20}{100}$ of potash in ulmin.

These experiments assign about $\frac{1}{5}$ for the quantity of potash in ulmin, but as it is impossible to operate, on so small a scale, on such substances without loss, it is probable that it even exceeds this proportion.

5. The substance separated from ulmin by acids has the following qualities:

It is very glossy, and has a resinous appearance.

In lumps it appears black, but in minute fragments it is found to be transparent, and of a garnet-red colour.

It burns with flame, and is reduced to white ashes.

Alcohol dissolves it, but only in very small quantity.

Water likewise dissolves it, but also only in very small quantity. Acids cause a precipitate in this solution, though this resin-like matter appears neither to contain any alkali, nor to retain any of the acid by means of which it was obtained.

Its solution in water seems to redden turnsol paper.

Neither ammonia, nor carbonate of soda, promote its solution in cold water.

On adding a small quantity of potash to water in which it

lies, it dissolves immediately and abundantly. This solution has all the qualities of a solution of ulmin, and, on exhalation, leaves a matter precisely like it, which cracks and separates from the glass, and does not grow moist in the air, &c.

Hence it appears that ulmin is not a simple vegetable principle of anomalous qualities, but a combination with potash of a red, or more properly a high yellow matter, which, if not of a peculiar genus, seems rather more related to the extractives than to the resins.

English Ulmin.

I collected, from an elm tree in Kensington gardens, a small quantity of a black shining substance which looked like ulmin.

It was readily soluble in water, and the solution was in colour and appearance exactly similar to a solution of ulmin.

This solution, exhaled to a dry state on a water-bath, left a matter exactly like ulmin, and which cracked and divided as ulmin does, when dried in the same manner. It did not, however, rise up from the watch-glass in long strips, like the Sicilian kind, but this may have been owing partly to its small quantity, which occasioned it to be spread very thin on the watch-glass, and partly to its containing a considerable excess of alkali, for it differed also from the Palermo ulmin by becoming soft in the air, and its solution strongly restored the blue colour of reddened turnsol paper.

Nitric acid, added to a filtered solution of this ulmin, immediately caused a precipitate in it, and the filtered solution, on evaporation, afforded numerous crystals of nitrate of potash.

This English ulmin made a considerable effervescence with acetous acid, which the Palermo ulmin had not been observed to do. This acetous solution, in which the acid was in excess, was exhaled dry, and repeatedly washed with spirit of wine. No part of the brown matter dissolved. Water dissolved this brown residuum readily and entirely. This solution did not sensibly restore the blue colour of reddened turnsol paper. Exhaled to a dry state, the matter left did not separate from the watch-glass quite as freely as Palermo ulmin, which had been treated with acetous acid; but it seemed no longer to grow moist in the air. Redissolved in water, and nitric acid added, the mixture became thick from a copious precipitate.

The spirit of wine contained a quantity of acetate of potash.

The excess of alkali, in this English ulmin, may be owing to the tree from which it was collected having been affected with the disease, which produces the alkaline ulcer to which the elm is subject.

Sap of the Elm Tree.

Thinking that the production of ulmin by the plant might not be the consequence of disease, and that it might exist in the healthy sap, a bit of elm twig, gathered in the beginning of last July, was cut into thin slices and boiled in water. It afforded a brown solution, like a solution of ulmin. Exhaled to dryness, this solution left a dark brown substance, in appearance similar to ulmin, but on adding water to this dry mass, a large quantity of brown glutinous matter remained insoluble. The mixture being thrown on a filter, a clear yellow liquor passed, which may have contained ulmin, but the quantity was too small to admit of satisfactory conclusions.

Perhaps older wood, the juice of which was more perfected, would afford other results, since ulmin appears to be the product of old trees; but the inquiry, being merely collateral to the object I had originally in view, was not persevered in.

VII. *On a Method of Freezing at a distance. By* William Hyde Wollaston, *M.D. Sec. R. S.*

Read December 17, 1812.

THAT a fluid, from which a portion is evaporated, becomes colder in consequence of the heat absorbed by that part which assumes the gaseous state: that fluids rise in the state of vapour at a lower temperature when the pressure of the atmosphere is removed, and consequently may be cooled to a lower degree by evaporation *in vacuo* than in the open air, are facts too well known to need confirmation before the Members of this Society by any new experiments.

Nevertheless, a new mode of applying the most established principles may deserve to be recorded, if it assist the illustration of them, and be instructive from the novelty of the view in which it exhibits a certain class of phenomena; although no immediate use be at present proposed, to which it can be applied with advantage.

If an attempt were made to freeze water by evaporation, without other means than the vacuum of an air-pump, the pump must be of the best construction, and though the quantity of water be small, the receiver must be of large dimensions, otherwise its capacity would set too confined a limit to the quantity of vapour that will rise, and consequently to the degree of cold produced.

Supposing the commonly received estimates to be correct,

as to the quantities of heat, that become latent in the conversion of ice into water, and of water into steam, being 140° and 960° respectively, we should find the following statement to be not far from the truth.

If 32 grains of water were taken at the temperature of 62°, and if one grain of this were converted into vapour by absorbing 960°, then the whole quantity would lose $\frac{960^\circ}{32} = 30^\circ$, and thus be reduced to the temperature of 32°.

If from the 31 grains, which still remain in the state of water, 4 grains more were converted into vapour by absorbing 960°; then the remaining 27 grains must have lost $\frac{4}{27}$ of $960^\circ = 142^\circ$, which is rather more than sufficient to convert the whole into ice. In an experiment, conducted upon a small scale, the porportional quantity evaporated did not much differ from this estimate.

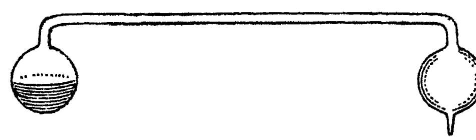
If it be also true, that water in assuming the gaseous state, even at a low temperature, expands to 1800 times its former bulk; then in attempting to freeze the small quantity of water abovementioned, it would be requisite to have a dry vacuum with the capacity of 5×1800, or equal to that of 9000 grains of water.

As a means of avoiding the necessity of so large a vacuum, Mr. LESLIE had recourse to the ingenious expedient of employing an extensive surface of sulphuric acid, for the purpose of absorbing the vapour generated in the course of the experiment, and by that means contrived to freeze much larger quantities of water, than could otherwise have been done, and by a far less laborious process.

But even in this method the labour is not inconsiderable,

and the apparatus, though admirably adapted to the purpose for which it is designed, is large and costly. I have therefore thought the little instrument I am about to describe may possess some interest, as affording a readier and more simple mode of exhibiting so amusing and instructive an experiment.

Let a glass tube be taken, having its internal diameter about $\frac{1}{8}$ of an inch, with a ball at each extremity of about one inch diameter; and let the



tube be bent to a right angle at the distance of half an inch from each ball. One of these balls should contain a little* water, and the remaining cavity should be as perfect a vacuum as can readily be obtained. The mode of effecting this is well known to those who are accustomed to blow glass. One of the balls is made to terminate in a capillary tube, and when water admitted into the other has been boiled over a lamp for a considerable time, till all the air is expelled, the capillary extremity, through which the steam is still issuing with violence, is held in the flame of the lamp till the force of the vapour is so far reduced, that the heat of the flame has power to seal it hermetically.

When an instrument of this description has been successfully exhausted, if the ball that is empty be immersed in a freezing mixture of salt and snow, the water in the other ball, though at the distance of two or three feet, will be frozen solid in the course of a very few minutes. The vapour contained in the empty ball is condensed by the common opera-

* If the ball be more than half full, it will be liable to burst by the expansion of water in freezing.

tion of cold, and the vacuum produced by this condensation gives opportunity for a fresh quantity to arise from the opposite ball, with proportional reduction of its temperature.

According to a theory that does not admit of positive cold, we should represent the heat of the warmer ball to be the agent in this experiment, generating steam as long as there remains any excess of heat to be conveyed. But if we would express the cause of its abstraction, we must say that the cold mixture is the agent, and may observe, in this instance, that its power of freezing is transferred to a distance, by what may be called the negative operation of steam.

The instrument, by which this is effected, may aptly be called a Cryophorus, which correctly expresses its office of frost-bearer.

VIII. *A Catalogue of North Polar Distances of some of the principal fixed Stars. By* John Pond, *Esq. Astronomer Royal, F. R. S.*

Read December 17, 1812.

The catalogue which I have the honour of transmitting to the Society, is the result of a great number of observations made with the new Mural Circle, from the month of June last to the present time.

I hope at no very distant period to add considerably to its precision; but I trust that in its present state, it will not be thought unacceptable to astronomers, as from the experience I have now obtained of the instrument, I may safely pronounce that it far exceeds in accuracy any thing yet known in the history of practical astronomy.

In the fourth column I have annexed the degree of uncertainty, or maximum of error, to which I conceive the results are liable. These numbers not being derived from any strict mathematical process, must only be considered as indicating the opinion I have formed on the subject. When the observations themselves are laid before the Society, I shall have an opportunity of stating at length on what grounds this opinion was formed.

	Names of Stars.	N. P. Distances begin. 1813.	No. of Observations.	Maximum of Error to which this Catalogue may probably be liable.
		° ′ ″		
1	Polaris (Summer (Winter	1 41 22,07 1 41 21,47	120	
2	β Ursæ Min.	15 4 48,95	50	0,50
3	β Cephei	20 5 30,30	20	0,50
4	α Ursæ Maj.	27 14 31,50	36	0,50
5	α Cephei	28 12 12,35	20	0,50
6	α Cassiop.	34 29 22,83	38	0,50
7	γ Ursæ Maj.	35 15 55,10	10	0,75
8	γ Draconis	38 29 3,73	68	0,25
9	η Ursæ Maj.	39 44 57,80	50	0,25
10	α Persei	40 48 52,37	26	0,50
11	Capella	44 12 20,96	37	0,50
12	α Cygni	45 22 57,19	40	0,50
13	α Lyræ	51 23 0,72	70	0,25
14	Castor	57 42 46,57	30	0,50
15	Pollux	61 31 56,57	31	0,50
16	β Tauri	61 33 43,51	23	0,50
17	α Andromedæ	61 56 29,61	35	0,50
18	α Cor. Bor.	62 38 55,70	53	0,50
19	α Arietis	67 25 36,76	44	
20	Arcturus	69 50 19,04	50	0,25
21	Aldebaran	73 52 35,18	30	0,50
22	β Leonis	74 22 57,24	24	0,50
23	α Herculis	75 23 13,97	23	0,50
24	α Pegasi	75 47 51,77	20	0,50
25	γ Pegasi	75 51 21,00	25	0,50
26	Regulus	77 7 22,94	30	0,50
27	α Ophiuchi	77 17 39,66	37	
28	γ Aquilæ	79 50 0,62	38	0,75
29	α Aquilæ	81 36 58,88	45	
30	β Aquilæ	84 3 4,09	12	1,0‡
31	α Orionis	82 38 16,11	21	0,50
32	α Serpentis	82 58 39,50	33	0,50
33	Procyon	84 18 15,03	20	0,50
34	α Ceti	86 39 0,75	18	1,0‡
35	α Aquarii	91 13 21,64	20	0,50
36	α Hydræ	97 51 11,30	10	0,75
37	Rigel	98 25 33,85	30	1,0‡
38	Spica Virginis	100 10 51,30	20	1,0
39	1 α Capricor.	103 4 35,45	35	
40	2 α Capricor.	103 6 52,32	28	
41	1 α Libræ	105 12 39,26	10	
42	2 α Libræ	105 15 22,69	15	
43	Sirius	106 28 0,70	34	
44	Antares	116 0 16,63	36	

Total No. of Observations 1452.

* α Aquilæ, α Arietis, and α Ophiuchi are probably determined within a second of the truth, but I have met with some discordances in these stars which I do not comprehend; future observations will shew, whether these are accidental or arise from some periodical cause.

‡ Those stars marked ‡ were determined by the quadrant, its error having been determined by comparison with the circle, by other stars having nearly the same declination.

IX. *A Description of the solvent Glands and Gizzards of the Ardea Argala, the Casuarius Emu, and the long-legged Casowary from New South Wales. By Sir* Everard Home, *Bart. F. R. S.*

Read December 17, 1812.

Having, upon a former occasion, laid before the Society an account of several varieties in the structure of the solvent glands and gizzards of birds, I now avail myself of some further materials, which I have procured since that time, to render the series more complete. The Ardea Argala, a native of Bengal, which feeds upon carrion, and is exceedingly voracious, has the solvent glands differently formed from those of any bird which I have examined; each gland is made up of five or six cells, and these open into one common excretory duct. The glands are disposed in two circular masses, one on the anterior, the other on the posterior surface of the cardiac cavity, putting on a similar appearance to those of the cormorant, but differing both in structure and situation. The gizzard of the Ardea Argala is lined with a horny cuticle, nearly of the same general appearance as that of the crow, and the digastric muscle is of similar strength. These parts are represented in the annexed drawing. (See Plate III.) This bird has a large bag hanging down on the forepart of the neck, which is rendered conspicuous by the skin that covers it being almost entirely without feathers, having only a few scattered on the most prominent depending part. The bag appears to

be readily filled and emptied at the will of the bird: upon examination after death, this bag was found to contain air, and to be unconnected with the organs of digestion, or the trachea which passes down along the middle of its cavity; it communicates by a large oval aperture with the air cells on the posterior part of the neck, and through them receives air from the lungs. The two species of casowary, Casuarius Emu, and the long-legged casowary from New South Wales, differ from one another in the form of their digestive organs, as well as in the length of their intestines.

In the Casuarius Emu, the solvent glands are oval bags one fourth of an inch long and only one-sixteenth of an inch wide; they occupy the whole surface of the cardiac cavity which is very large, and they are all placed nearly in a transverse direction respecting the cavity, the orifices of the excretory duct appearing very distinctly through the membrane which lines the cardiac cavity. The gizzard is nearly of the strength of that of the crow, but has a thicker cuticular lining. This cuticle extends beyond the cavity of the gizzard both above its orifice and downwards towards the duodenum. The most remarkable circumstance respecting the gizzard, is its being situated out of the direction of the cardiac cavity, forming a pouch on the posterior part, and having before it an open channel lined with cuticle, along which the food can readily pass into the duodenum without being received into the gizzard, as in other birds. At the commencement of the duodenum there is a broad valve, upon the edge of which the cuticular lining of the cavity of the gizzard terminates. The duodenum, at its origin, swells out into an oval cavity four inches long, and two and an half in diameter; it then contracts to one inch and a half in dia-

meter, and the intestine afterwards continues nearly of that size.

In the long-legged casowary from New South Wales, the solvent glands are of the same length as those of the Emu, but are twice the width; they occupy the whole surface of the cardiac cavity, in the middle line of which there is a row of these glands in the direction of the cavity, and on the two sides the glands are arranged in an oblique direction towards this line. The gizzard is rather stronger than in the Emu, but resembles it in all other respects. The duodenum at its origin is by no means so large.

The annexed drawings (See Plates IV. V.) shew all the parts in so distinct a manner, as to make a more particular description of them unnecessary; and I feel myself much indebted to the kindness of Mr. Howship, who, in the absence of Mr. Clift, took the trouble of making them. His knowledge of comparative anatomy has enabled him to execute them in a much more satisfactory manner than could have been done by any artist who was not at the same time an anatomist.

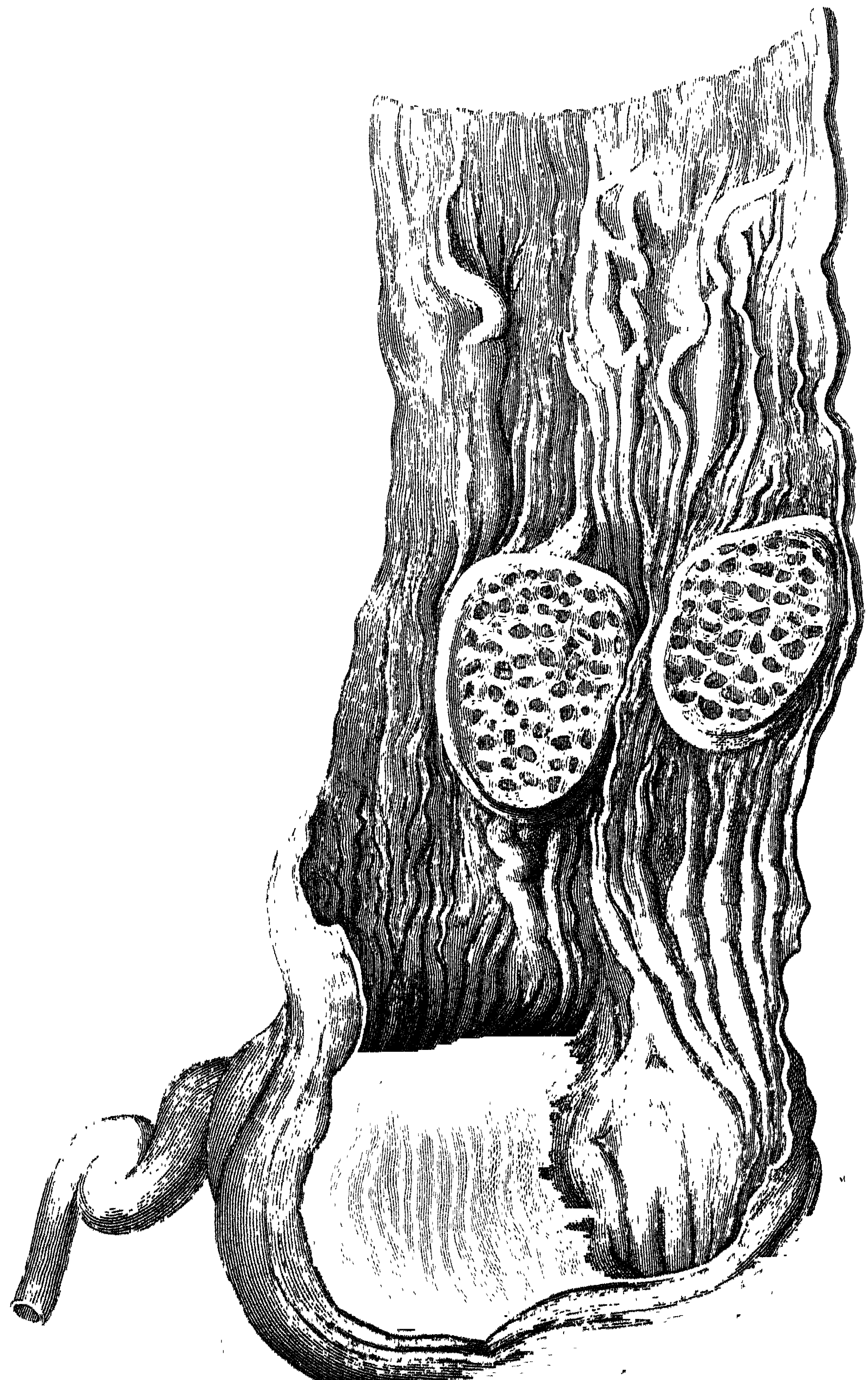
It is a very interesting part of comparative anatomy to examine the digestive organs belonging to birds, between which there is a similarity in size, food, and habits of life; and to trace the varieties in structure by means of which each species is enabled to subsist upon the produce of the country, of which it is the natural inhabitant, with every possible advantage. This cannot be better illustrated than by the following examples.

The Casuarius Emu, a native of Java, one of the most fertile and luxuriant islands in the world, has the solvent glands

of a very small size; the gizzard so situated as only to be used occasionally; the small intestines five feet long, two inches in diameter, and the great intestine one foot long, making in all six feet; and two cæca, each of them six inches in length, one-fourth of an inch in diameter. The Casowary of New South Wales, a less fertile country than Java, has larger solvent glands, a stronger gizzard placed under the same circumstances; the small intestines one inch and a quarter in diameter and ten feet seven inches long; the large, two inches in diameter, thirteen inches long, in all thirteen feet; the cæca two inches long, and half an inch in diameter.

The Rhea Americana, a native of South America, where its food is not abundant, has solvent glands of an unusually complex form, and a strong gizzard, so placed that every part of the food must be triturated in it. The small intestines one inch in diameter, ten feet seven inches long; the large intestine one inch and a quarter in diameter, one foot eight inches long, in all twelve feet three inches; but to these are added two cæca, each of them three feet ten inches, and two inches in diameter at the widest part.

The Struthio Camelus of Africa, which in the Desert has a very precarious subsistence, has solvent glands of the same structure as in the Rhea Americana, but much more numerous; a much stronger gizzard, so situated that the food must undergo a previous trituration before it can arrive at it; the small intestines one inch and a half in diameter, and twenty-seven feet long; the large intestine two inches in diameter, and forty-five feet three inches long, in all seventy-two feet three inches; and two cæca three inches in diameter at the widest part, and each two feet nine inches long.



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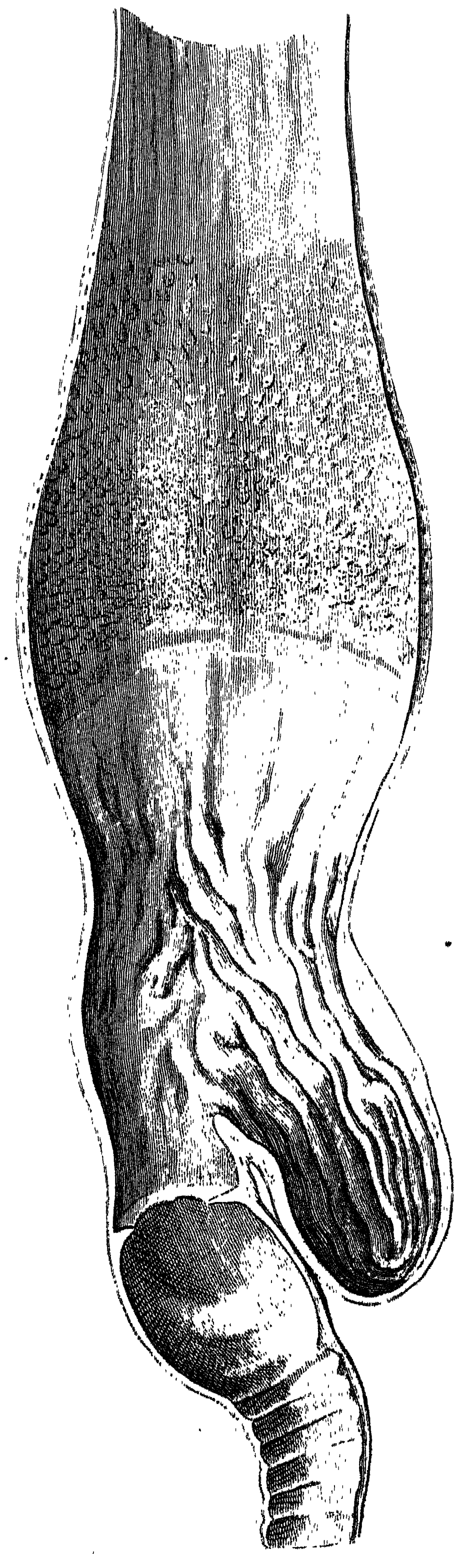


J. Howship del. J. Basire sc.

In this series of structures, we have not only the gizzard becoming more and more fitted to economize the food as the country becomes less fertile, but we have also an extension of the lower intestines, and cæca, to such a degree as to lead to the belief that the processes carried on in them, render the undigested food subservient to the animal's support.

X. *Additional Remarks on the State in which Alcohol exists in fermented Liquors.* *By* William Thomas Brande, *Esq. F.R.S.*

Read December 17, 1812.

THE experiments and observations contained in this paper, are intended as supplementary to a communication on the same subject, which the Royal Society has done me the honour to insert in the Philosophical Transactions for the year 1811.*

On that occasion, I endeavoured to refute the commonly received opinion respecting the *production* of alcohol during the distillation of fermented liquors, by shewing, that the results of the process are not affected by a variation of temperature equal to twenty degrees of FAHRENHEIT's scale; that is, that a similar quantity of alcohol is afforded by distilling wine at 180° and at 200°.

I also conceived that any new arrangement of the ultimate elements of the wine, which could have given rise to the formation of alcohol, would have been attended with other symptoms of decomposition, that carbon would have been deposited, or carbonic acid evolved, which in the experiments alluded to, was not the case. Upon such grounds I ventured to conclude, that the relative quantity of alcohol in wines, might be estimated by submitting them to a careful distillation, and by ascertaining the specific gravity of the distilled liquor with the precautions which I have formerly described.

* Page 337.

This conclusion may be objected to, by supposing that the lowest temperature, at which the distillations were performed, was sufficient for the formation of alcohol from the elements existing in the wine; but it is not easy to conceive how this should happen, without some of those other changes which I have just noticed.

It has been stated, in my former paper, that the separation of alcohol from wine, by the addition of subcarbonate of potash, is prevented by the combination of the alkaline salt with the colouring-extractive, and acid contained in the liquor. I have also shortly noticed some unsuccessful attempts to separate these substances by other means than distillation.

In prosecuting the inquiry, this difficulty has been surmounted, and I shall proceed to shew, that alcohol may be separated from wine without the intervention of heat, and that the proportion thus afforded is equal to that yielded by distillation.

When the acetate,* or subacetate† of lead, or the subnitrate of tin‡ are added to wine, a dense insoluble precipitate is quickly formed, consisting of a combination of the metallic oxide, with the acid and colouring-extractive matter of the wine, and when this is separated by filtration, a colourless fluid is obtained, containing alcohol, water, and a portion of the acid of the metallic salt, provided the latter has not been added in excess, in which case a part remains undecomposed.

* Sugar of lead.

† Formed by boiling two parts of sugar of lead with one of finely powdered litharge, in six parts of water. The solution should be preserved in well closed phials, as it is rapidly decomposed by attracting carbonic acid from the atmosphere. Even while hot, a poriton of carbonate of lead is formed in it.

‡ Prepared by dissolving protoxide of tin in cold dilute nitric acid.

The acetate of lead and the subnitrate of tin produce the desired effect of separating the colouring and acid matters, in the greater number of instances, but they are less rapid and perfect in their action, and not so generally applicable as the subacetate of lead,* which is the substance that I commonly employed.

The following experiment was made with a view to ascertain the effect of this salt.

Twenty measures of alcohol, specific gravity ,82500, were mixed with eighty measures of distilled water coloured with log wood, and rendered slightly acid by supertartrate of potash. Four measures of a concentrated solution of the subacetate of lead were added to this mixture, and the whole poured upon a filter. A precipitate was thus collected of a deep purple colour, which appeared to consist of oxide of lead combined with tartaric acid and the colouring-extractive matter.

The filtered liquor was perfectly transparent and colourless, and afforded, on the addition of subcarbonate of potash, 19,5 measures of alcohol.†

* The effect of this salt upon colouring matter, was first pointed out to me by Mr. E. M. NOBLE of Chelsea.

† Pure subcarbonate of potash, obtained by igniting the carbonate, was employed in these experiments. I found that about 19,5 parts of alcohol were separated in the course of four hours, by the addition of 50 parts of the subcarbonate to a mixture of 20 parts of alcohol by measure with 80 of distilled water, and that no further separation took place. The alcohol is always slightly alkaline, probably from containing a small portion of the solution of the subcarbonate, or of pure soda, but as this did not interfere with the object of the experiment, it was not particularly attended to.

When the subcarbonate was added to a mixture of four parts by measure of alcohol with 96 of water, no separation was effected.—A mixture containing 8 per cent. of alcohol afforded about 7 parts—one containing 16 per cent. about 15,5, and where the proportion of alcohol exceeded 16 per cent. the quantity, indicated by the action

Finding that the separation of alcohol by subcarbonate of potash from mixtures of spirit and water, was nearly complete, and that colouring-extractive matter, and tartaric acid might be removed from such mixtures by the subacetate of lead, I proceeded to examine wine by such modes of analysis.

The following results were obtained by these, and other comparative experiments.

1. One part by measure of a concentrated solution of subacetate of lead, was added to eight measures of common port wine: the mixture having been agitated for a few minutes, was poured upon a filter.—The filtrated liquor was perfectly colourless, and the addition of dry subcarbonate of potash effected a rapid separation of alcohol.*

100 measures of the wine thus treated, afforded 22,5 measures of alcohol.

2. Eight ounces of the wine employed in the last experiment, were distilled in glass vessels, as described in my former paper.—The specific gravity of the distilled liquor at the temperature of 60° was 0,97530, which indicates 22,30 per cent. by measure of alcohol of the specific gravity of ,8250.

3. Eight ounces of the same wine were introduced into a retort placed in a sand heat, and the process of distillation was stopped when six ounces had passed over into the receiver.

of the subcarbonate, was always within 0,5 per cent. of the real proportion contained in the mixture. So that in the examination of wines containing less than 12 per cent. of alcohol, the method described in the text is somewhat exceptionable. The above experiments were made in glass tubes varying in diameter from 0,5 inch to 2 inches, and accurately graduated into 100 parts.

* When any excess of the subacetate had been employed, a portion of carbonate of lead was thrown down; but this did not interfere with the subsequent separation of the alcohol.

After the vessels were completely cooled, the portion in the receiver was added to the residuum in the retort. The specific gravity of this mixture (ascertained with proper precautions) was ,9884, that of the original wine = 0,9883.*

When care was taken to prevent the escape of vapour, no change of specific gravity was produced in the wine by three repetitions of the above process.

Similar experiments were repeated upon Madeira, Sherry, Claret, and Vin de grave, wines differing in the relative proportions of alcohol, colouring matter, and acid which they contain, and the results were as decisive; so that I conceive it is amply proved, by experimental evidence, that no alcohol is *formed* during the distillation of wines, and that the whole quantity found, after distillation, pre-existed in the fermented liquor.

It has been frequently asserted, that a mixture of alcohol and water, in the proportions I have stated them to exist in wine, would be much more effectual in producing intoxication, and the general bad effects of spirituous liquors, than a similar quantity of the wine itself. But this is true to a very limited extent only: when brandy is added to water, it is some time before the two liquids perfectly combine, and with alcohol this is more remarkably the case, and these mixtures are warmer to the taste, and more heating, if taken in this state of imperfect union, than when sufficient time has been allowed for their perfect mutual penetration.

I have also ascertained that distilled port wine tastes stronger, and is more heating than the wine in its original state, and that these qualities are impaired, and the wine reduced nearly

* This experiment was suggested in the Edinburgh Review for November, 1811.

to its original flavour, by the addition of its acid and extractive matter. With claret, and some other wines, containing less alcohol and more acid than port, these circumstances are more readily perceived; and lastly, if the residuum afforded by the distillation of 100 parts of port wine, be added to 22 parts of alcohol and 88 of water (in a state of perfect combination), the mixture is precisely analogous in its intoxicating effects to port wine of an equal strength.

In the table annexed to my former paper, it appears that the average quantity of alcohol contained in port wine amounts to 23,48 per cent.; but two of the wines there alluded to are stronger than any I have since met with, and were, at that time, sent to me as "remarkably strong and old port." I have lately examined a number of specimens of the better kinds of port wine in common use, and the results of these experiments lead me to place the average strength at 22 per cent. of alcohol by measure.

A port wine procured for me by Dr. BAILLIE, and to which no brandy had been added, afforded 21,40 per cent. of alcohol: another specimen of a similar description, put into my hands by an Oporto merchant, contained only 19 per cent.; it is the weakest port wine I have met with.

The other results given in the table, agree perfectly with those of subsequent and more extended experiments.

XI. *On a new Variety in the Breeds of Sheep. By Colonel* David Humphreys, *F. R. S. In a Letter to the Right Hon. Sir* Joseph Banks, *Bart. K. B. P. R. S.*

Read January 14, 1813.

Sir,

Humphreysville (in the State of Connecticut), Nov. 1, 1811.

I propose to give some account of a new variety in the breeds of sheep, which has lately sprung up in America.

Seth Wight, who possessed a small farm on the banks of Charles river, in the town of Dover and State of Massachusetts, about sixteen miles distant from Boston, kept a little flock composed of fifteen ewes and one ram. In the year 1791, one of the ewes produced a lamb of singular appearance. By the advice of some of his neighbours, he killed his former ram, and reserved the young one for breeding. The first season, two lambs only were yeaned in his likeness. In the following years, a number more, distinguished by the same peculiarities. Hence proceeded a strongly marked variety in this species of animals, before unknown in the world. It has been called by the name of the *Otter breed.*

This name was given from a real or imaginary resemblance to that animal, in the shortness of the legs and length of the back; by some supposed to have been caused by an unnatural intercourse; by others, perhaps as fancifully, from fright during gestation. It is only certain, that otters were then

sometimes seen on the banks of this river. They have since disappeared.

The person, who was the first to dissect one of these sheep for the purpose of ascertaining the properties and qualities which distinguish them from our common breed, has added the appropriate term of ANCON.

The singularity of form seems to be confirmed in the blood. Experiments, in crossing, have changed the strain, or, if I may be allowed so to express it, amalgamated the qualities of this with those of other breeds, so as to produce a mixed or mongrel race, in too few instances to form an exception to the theory.

When both parents are of the otter or ancon breed, the descendants inherit their peculiar appearance and proportions of form. I have heard of but one questionable case of a contrary nature.

The small number of cases where the young are said to partake in part, but not altogether, the characteristics of this breed, will not invalidate the general conclusions, established on experience in breeding from a male and female of distinct kinds.

When an ancon ewe is impregnated by a common ram, the increase resembles wholly either the ewe or the ram.

The increase of a common ewe, impregnated by an ancon ram, follows entirely the one or the other, without blending any of the distinguishing and essential peculiarities of both.

The most obvious difference between the young of this and other breeds, consists in the shortness of the legs of the former; which combined with debility or defect of organization, often makes them cripples in maturer age.

Frequent instances have happened where common ewes have had twins by ancon rams, when one exhibited the complete marks and features of the ewe; the other of the ram. The contrast has been rendered singularly striking when one short legged and one long legged lamb, produced at a birth, have been seen sucking the dam at the same time.

The facts respecting the fleeces have not been so well ascertained. They have been judged by some to be finer and heavier than those of our common breed; by others, of a medium fineness, but possessing more uniformity of pile on the same, and on different sheep of this kind. I have seen instances of their varying considerably from each other.

One case, where the young assumed the perfect likeness of the ewe, together with a meliorated pile apparently derived from the ram, is too interesting to be omitted. The inclosed specimen of wool, No. 1, is from an ancon-Merino: that is to say, the offspring of an ancon ewe and Merino ram. Its shape is the very image of the former: its wool, which covers almost the whole face, and extends quite down to the fetlocks, of a pretty fine quality (a common sign of the best blooded Merinos) partakes the silky feel and felting quality of the latter; with, I judge, about the same portion of fineness as the fleeces, which my quarter-blooded Merinos ordinarily carry. The locks, No. 2, 3, and 4, were clipped from a wether, ram and ewe descended immediately from ancon parents on both sides. The fleece of the former weighed four pounds and a half: those of the two latter somewhat rising three pounds each.

The ancons have been observed to keep together, separating themselves from the rest of the flock, when put into inclosures with other sheep.

The lambs are remarked to be less capable of standing up to suck without assistance, when first yeaned, than others.

Although they arrive somewhat later at maturity, the sheep are said to live as long as those of our common breeds; unless in some cases, where by reason of their debility and decrepitude, their health is impaired and their lives shortened.

To whatever cause it may be attributed, whether arising from defect in vertebræ, muscle, joint, or limb, it is certain that they can neither run nor jump like other sheep. They are more infirm in their organic construction, as well as more awkward in their gait, having their fore-legs always crooked, and their feet turned inwards when they walk. According to some information, the rams are commonly more deformed than the ewes.

Sprung from an individual, remarkable for what might be called a caprice of nature,* it is not one of the least extraordinary circumstances, that this misshapen and feeble race should propagate their own deformity and decrepitude until these characteristics have become constitutional and hereditary.

It may be asked with reason, why such a breed should have been continued?

The expectation of advantage, particularly in one way, doubtless prevailed over slighter considerations. We cannot boast of being such neat farmers, or of being so much attached to fine shapes in animals as the more skilful graziers and breeders in Europe; consequently the prospect of gain in some useful quality, or even of exemption from inconvenience, would more readily recompense us for the want of beauty, or reconcile us to the sight of what, to more acute or

* Lusus naturæ.

fastidious spectators, might be considered its opposite. The unfavourable appearance of Merinos, according to the generally received ideas of handsome proportions in sheep, is understood to have operated considerably in retarding their spread in France and England, as well as in a smaller degree in the United States of America.

The breed of ancons was expected to be a valuable acquisition, on account of their being less able than others to get over fences.

In New England, beyond which they have rarely migrated, there are few commons: no hedges: no shepherds: and no dogs, whose business it is to watch flocks. The small freehold estates are enclosed by fences of wood or stone. These are frequently too low to prevent active sheep from breaking out of pastures, into meadows, or grounds under cultivation. Crops are injured. Farmers discouraged. Hopes were entertained that this evil would be remedied. It has been in part.

To countervail this advantage, the drovers have complained of the great difficulty of driving these cripples to market; and the butchers, that the carcase is smaller and less saleable, than that of our common breeds. Perhaps, it is commonly not so fat. I have perceived little difference in the taste of the mutton; and presume, if served at table in equal condition, it would hardly be distinguished by better judges. They have been remarked not to fatten so easily, possibly owing to less facility or industry in gathering food, or to some fault in the organic system.

Since the introduction of Merinos, which are equally gregarious, quiet, and orderly, probably better feeders, and with greater disposition to take fat, and more highly recom-

mended by their fleeces, the ancon breed seems in danger of becoming almost extinct. They have so much declined, that, for many months, it was not an easy matter for me to procure one for dissection in Boston. That operation was performed by the ingenious Dr. SHATTUCK, who makes the following remarks.

"The sheep weighed just before it was killed forty-five "pounds. The most obvious difference in its skeleton from "the skeleton of the common sheep, so far as my superficial "observation has extended, consists in the greater looseness "of the articulations, the diminished size of the bones; but "more especially in the crookedness of its forelegs, which "causes them to appear like elbows, while the animal is walk-"ing. I have taken the liberty to call them *ancon*, from the "Greek word which signifies elbow. On dissecting the sheep, "I could not forbear noticing the comparatively flabby condi-"tion of the subscapularic muscles: this may partially account "for the great feebleness of the animal, and its consequent "quietude in pasture."

This skeleton will be presented to the President of the Royal Society, by the hand of the gentleman who is so obliging as to charge himself with the delivery of this letter.

I have been the more particular in the statement, because I deemed it important the point should be settled, so far as evidence can be adduced, that the preservation of different breeds, once clearly designated, in whatever manner obtained, whether from casualty, as in the present instance, or from calculation and cultivation, as in that of the new Leicester breed, depends more on some inherent quality in the blood, than on climate, food, or any other circumstance. Although it is allowed

that these have no inconsiderable influence, particularly the first, on the fleece, in the torrid zone. In all temperate regions, and even in the higher latitudes, where extreme cold prevails, flocks may be improved by care, or deteriorated by the want of it.

The settlement of this point would not fail to have a tendency to eradicate the remains of the pernicious prejudice, that the Merinos of Spain cannot be bred out of that country, without degenerating and losing their essential character for wool.

The beneficent Creator having ordained "that all creatures shall increase after their kinds," has still left much for man to do, in regard to those which are made more immediately subservient to his use.

We are not ignorant how much the agricultural nations of Europe and America are indebted for meliorations in their husbandry to modern researches and discoveries in chemistry, natural history, and other branches of philosophy; as well as to experiments of eminent farmers, and especially breeders of cattle.

My experience has been too limited for me to flatter myself with being able to add to the stock of materials for investigation and improvement, except by becoming in some degree the medium of communication between the agriculturalists of the two continents.

I have formerly exerted myself to enable my countrymen to improve their breeds of useful animals, perhaps not altogether without success. My present object should rather be, to supply facilities and inducements for abler men, possessed of better opportunities, to discover and disclose the best means

for selecting and spreading the most approved breed of fine woolled sheep, by which Merinos are meant, throughout the different countries which are known to be well adapted to their cultivation.

So tempting a motive for contributing my mite to the repertory of a Society, justly celebrated for the extension of human knowledge and improvements, and which has done me the honour to enrol my name in their number, was not to be resisted; and the less, as it affords me occasion of presenting, at the same time, the homage of high respect for their President, with which I have the honour to be,

Sir, your most obedient servant,

D. HUMPHREYS.

XII. *Experiments to ascertain the coagulating Power of the Secretion of the gastric Glands. By Sir* Everard Home, *Bart. F. R. S. Communicated by the Society for promoting the Knowledge of Animal Chemistry.*

Read January 21, 1813.

In attempts to investigate the process of digestion in quadrupeds, the difficulties are almost insurmountable; the gastric glands are scarcely perceptible, and occupy a small portion of the stomach, every other part of the inner membrane is throwing out secretions of a different kind, and these are all mixed together with the food in the general cavity. Under such circumstances, the properties of the secretion of the gastric glands can never be ascertained, since it cannot be procured in a pure state. It is generally allowed, that the first process the food undergoes in the stomachs of animals is being converted into a jelly; but whether this is produced by the gastric liquor as the previous change to dissolution, or whether it takes place before that liquor is applied, has not been ascertained.

Mr. Hunter made many experiments upon the coagulating power of the secretions of the stomach, which establish the fact of coagulation taking place in the stomachs of animals of different classes.

An infusion of the dried inner membrane of the fourth cavity of the stomach of the calf, being in common use for

the purpose of coagulating milk, proves, that every part of that membrane possesses such a power.

Mr. Hunter ascertained, that the mucus found in the first, second, and third cavity of the calf's stomach, dissolved in water, had no power of coagulating milk, while a solution of the mucus of the fourth cavity possessed that property, and retained it even after it had been so long kept, as to begin to become putrid.

When the calf has left off sucking, and is old enough to be killed for veal, the inner membrane of the fourth cavity of the stomach readily coagulates milk.

If different portions of the inner membrane of the hog's stomach are prepared as rennet, no part coagulates milk but that near the pylorus, and I have shewn, in a former paper, that the gastric glands are situated there.

The crop and gizzard of a cock were prepared as rennet, milk was coagulated in half an hour by that of the gizzard, in two hours by that of the crop.

The contents of a shark's stomach coagulated milk immediately; portions of the stomach washed and steeped sixteen hours in water, formed a solution which produced the same effect.

Rennet made from the stomach of a salmon coagulated milk in four or five hours; when made from the stomach of the thornback, it produced the same effect.

These experiments shew, that the secretions of the stomach have a power of coagulating milk; they do not, however, explain whether this power belongs to any one secretion in particular, or to a mixture of them all.

With a view to ascertain to what particular secretions this

property belongs, I instituted the following experiments, which were made by my fellow labourers in animal chemistry, Mr. HATCHETT and Mr. W. BRANDE.

On the 9th of June, 1812, Mr. HATCHETT took the cardiac portion of a chicken's stomach with the gastric glands that open into it, and put it into a glass vessel; the horny lining of the gizzard was put into another, milk was added to each, and was converted into a curd; but the curd in the vessel containing the lining of the gizzard was the firmest.

Mr. BRANDE, on the 12th of June, 1812, made a similar experiment with the cardiac portion of the stomach of the hawk, as a carnivorous bird, in one vessel; and the cardiac portion of the fowl, as a granivorous one, in another. The coagulating power of the hawk's stomach was found to be the most powerful.

To ascertain whether the coagulating power belongs to the secretion of the gastric glands, and is only communicated to the other parts, I instituted the following experiment, which was made by Mr. BRANDE on the 13th of July, 1812. I selected the turkey for the subject of it, as the gastric glands of that bird are larger than in most others. The turkey had been kept one day without food before it was killed, and immediately after death the gastric glands were very carefully dissected from each other, on the outside of the membrane which lines the cardiac cavity without opening into it, each gland was then separately removed by cutting through the excretory duct, leaving the cardiac cavity entire and unopened.

Of these glands slit open 40 grains were put into a vessel, and two ounces of new milk added.

40 grains of rennet were put into a second vessel, to which the same quantity of milk was added.

40 grains of the lining of the cardiac cavity of the turkey, and the same quantity of milk were put into a third vessel.

40 grains of the fourth cavity of a calf's stomach in a recent state, with the same quantity of milk, were put into a fourth vessel.

The experiment commenced at ten o'clock in the morning, at half past ten the milk with the rennet became thick, at twelve curd was formed, at two whey separated, at four the formation of curds and whey appeared complete.

At half past eleven the milk with the glands became thick, at one curd was formed, at three whey separated, at six the formation of curds and whey was complete.

The milk with the portion of the recent calf's stomach underwent the same changes at the same periods.

The milk with the cardiac membrane of the turkey at four became thick, at eight curd was formed, the separation into curds and whey was not complete till next morning.

A portion of the same milk in twenty-four hours had undergone no change, except that cream had separated.

The rennet in a dried state consisted of four times the quantity of membrane employed in the experiment with the recent calf's stomach, which accounts for its more readily producing coagulation.

From these experiments, it is clear that the secretion of the gastric glands possesses the power of coagulating milk, and gives that power to all the parts by which it is imbibed, whether composed of living parts or not, since the horny lining

of the gizzard, the mucus in the stomach, and the inner membrane of that cavity appear equally to have acquired it.

This coagulation appears to be the first change the food undergoes in the process of digestion, and where the digestion is rapid, the coagulated parts are very quickly dissolved.

Mr. Bullock was led, by his love of natural history, to spend some time on the Bass Rocks, and has frequently seen a Solan goose swallow a herring, and come immediately to feed its young, and although the time the herring remained in its stomach could not have been more than a few minutes, when it was brought up again, to be given to the young bird, the external covering was entirely dissolved.

XIII. *On some Properties of Light. By* David Brewster, *LL.D. F.R.S. Edin. In a Letter to Sir* H. Davy, *LL.D. F.R.S.*

Read January 28, 1813.

Dear Sir,

Having been for some time engaged in a series of experiments on the phenomena of light arising from its transmission through diaphanous bodies, I have taken the liberty of communicating to you, for the information of the Royal Society, a short and general account of the results of my enquiries. In the narrow compass of a letter, it would be impracticable to include the various details of these experiments; the particular methods of observation that were employed; or the numerical results which I have obtained for the refractive and dispersive powers of nearly two hundred substances. As these will form part of a separate work, in which I am now engaged, I shall confine myself at present to some of those results which appear to be most interesting, either from their novelty or importance.

1. *On a new Property of refracted Light.*

As you are already well acquainted with the optical properties of doubly refracting media, and the analogous property of reflected light discovered by Malus, it will be unnecessary to take any notice of these phenomena. After repeating the experiments of Malus, and measuring several of the angles

of incidence at which this property was communicated to light by reflection from different substances, I made a variety of experiments, with the view of discovering if a similar character could be impressed upon light by its *transmission* through bodies, either wholly or imperfectly transparent. All these experiments afforded no new result, and every hope of discovering such a property was extinguished, when my attention was directed to a singular appearance of colour in a thin plate of agate. This plate, bounded by parallel faces, is about the fifteenth of an inch thick, and is cut in a plane perpendicular to the laminæ of which it is composed. The agate is very transparent, and gives a distinct image of any luminous object; but on each side of this image is one highly coloured, forming with it an angle of several degrees, and so deeply affected with colour that no prism of agate, with the largest refracting angle, could produce an equivalent dispersion. Upon examining this coloured image with a prism of Iceland spar, I was astonished to find that it had acquired the same property as if it had been transmitted through a doubly refracting crystal, and upon turning the Iceland spar about its axis, the images alternately vanished at every quarter of a revolution. My attention was now directed to the common colourless image formed by pencils transmitted perpendicularly through the agate; and by viewing it through a prism of Iceland spar, it exhibited all the characters of one of the pencils produced by double refraction, the images alternately vanishing in every quadrant of their circular motion.

When the image of a taper reflected from water at an angle of 52° 45′, so as to acquire the property discovered by Malus, is viewed through the plate of agate, so as to have its laminæ

parallel to the plane of reflection, it appears perfectly distinct; but when the agate is turned round, so that its laminæ are perpendicular to the plane of reflection, the light which forms the image of the taper *suffers total reflection, and not one ray of it penetrates the agate.*

If a ray of light incident upon one plate of agate is received after transmission upon another plate of the same substance, having its laminæ parallel to those of the former, the light will find an easy passage through the second plate; but if the second plate has its laminæ perpendicular to those of the first, the light will be wholly reflected, and the luminous object will cease to be visible.

Owing probably to a cause which will afterwards be noticed, there is a faint nebulous light unconnected with the image, though always accompanying it, and lying in a direction parallel to the laminæ. This light never vanishes along with the images, though it is evidently affected by the different changes which they undergo; and in one of the specimens of agate, it is distinctly incurvated, having the same radius of curvature with the adjacent laminæ. This character of the nebulous light I consider as an important fact, which may be the means of conducting us to a satisfactory theory, and I am at present engaged in examining it with particular care.

This remarkable property of the agate I have found also in the kindred substances of cornelian and chalcedony, and it is exhibited in its full effect even when these bodies are formed into prisms, and when the incident rays fall with any angle of obliquity. In one specimen of agate, which has no veins to indicate the direction in which it was cut, the images did not vanish as before; and in another specimen of the same

character the images suffered only an alternate diminution of brightness, in the same manner as a pencil of light receives only a partial modification when reflected from water at a greater or a less angle than 52° 45′.

The different experiments which have now been mentioned were repeated, with the most satisfactory results, by Mr. PLAYFAIR, Dr. HOPE, and Mr. JOHN DAVY.

Although the preceding results are by no means ripe for generalization, I cannot omit the present opportunity of hazarding a few conjectures respecting the cause of this singular property of the agate.

May not the structure of this mineral be in a state of approach to that particular kind of crystallization which affords double images? and may not the nebulous light be an imperfect image arising from that imperfection of structure? When one of the images vanishes, the nebulous light which encircled it is then a maximum, and it gradually diminishes during the re-appearance of the image. When the image which had disappeared recovers its full lustre, the surrounding nebulosity is very small, and this remaining light is, in all probability, no portion of the unformed image, but merely a few scattered rays arising from the imperfect transparency of the mineral.

By forming the agate into a prism, the nebulous light should be separated from the image which it encloses, in proportion to the angle contained by the refracting planes; but owing, perhaps, to the smallness of its double refraction, if it has such a property, I have not observed any separation of this kind.

The incurvated form of the nebulous light corresponding with the curvature of the laminæ, seems to connect it with the

laminated structure of the agate, and to indicate that the phenomena of double refraction are produced by an alternation of laminæ of two separate refractive and dispersive powers. In Iceland spar, one set of the laminæ may be formed by a combination of oxygen and calcium, while the other set is formed by a combination of oxygen and carbon. In chromate of lead, the chromium and oxygen may give one image, while the oxygen and lead give another. In like manner the carbonate of lead, the carbonate of strontites, jargon, and other crystals may give double images, in virtue of similar binary combinations. Of the simple inflammable bodies, sulphur is the only one which has the property of double refraction, but it will probably be found that it holds a metal or some other ingredient in its composition, which chemists have not been able to discover.

If the explanation which has now been given of the polarising power of the agate should be confirmed by future experiments, this property will be considered as a case, though a very curious one, of double refraction; but if these conjectures should be overturned, the phenomena which we have described must be ranked among the most singular appearances in the wide range of optical science.

2. *On the double Refraction of Chromate of Lead.*

In the course of my experiments on refractive powers, I discovered a double refraction in this metallic salt of such enormous magnitude, that the deviation of the extraordinary ray is more than thrice as great as that produced by Iceland spar. The ratio of the sines, for both refractions, and the other

properties of this extraordinary mineral will be noticed in the next article.

3. *On Substances with a higher refractive Power than the Diamond.*

Since the time of Sir ISAAC NEWTON, who first measured the action of the diamond upon light, its refractive power has been regarded as superior to that of every other substance; but, in the course of my researches, I have found that realgar and chromate of lead exceed the diamond in refractive power, and that this high refraction, in both these substances, is accompanied with dispersive powers greater than those of any other body. The following are the measures which I have obtained for these, and a few other substances.

Refractive Powers.

	Index of Refr.		Index of Refr.
Chromate of lead (gr. refr.) - -	2.926	Phosphorus - -	2.224
Ditto, least refraction	2.479	Sulphur, native -	2.115
Realgar - -	2.510	Cryolite - -	1.344
Diamond (according to NEWTON) - -	2.439	Ice - -	1.307

Dispersive powers or values of $\frac{dR}{R-1}$.

Chromate of lead (gr. refr.) - -	0.400	Phosphorus - -	0.128
Ditto, least refraction	0.262	Flint glass (highest)	0.052
Realgar - -	0.255	Diamond -	0.038
Oil of cassia -	0.139	Water - -	0.035
Sulphur - -	0.130	Fluor spar - -	0.022
		Cryolite - -	0.022

It appears from the first of these tables, that phosphorus

is next to diamond in refractive power, and that the three simple inflammable substances have their refractive powers in the order of their inflammability. Dr. WOLLASTON has placed phosphorus below horn and flint glass,* but I am confident that this distinguished philosopher, to whom the physical sciences are so deeply indebted, will find, upon making the experiment with prisms or lenses, that I have assigned the right place to that remarkable substance. The difference between the extreme dispersive powers in the second table is very remarkable, and the result for oil of cassia indicates in that body the existence of some ingredient, which chemical analysis has not been able to detect.

4. *On the Existence of two dispersive Powers in all doubly refracting Crystals.*

It has been long known, and it is indeed obvious, from a simple inspection of the images formed by a prism of Iceland crystal, that the one image is more coloured than the other, or that the actual *dispersion* of the one refraction is greater than the *dispersion* of the other, in the same manner as the *dispersion* of a prism of flint glass with a refracting angle of 12 degrees, is greater than the dispersion of a prism of the same glass with an angle of only 10 degrees.

Dr. WOLLASTON, who was the first person that examined the subject of dispersive powers with philosophical accuracy, makes the dispersive power of Iceland spar considerably above water, and even above diamond. Upon repeating this experiment, with the least refracted image, I found the dispersive

* Dr. WOLLASTON is satisfied that his original estimate was erroneous, and that Dr. BREWSTER's determination is very near the truth. H. D.

power, or the value of $\frac{dR}{R-1}$, to be 0.026 very considerably below water, which stands at 0.035 of the scale, and I therefore concluded that Dr. Wollaston had examined the greatest refraction, while I had examined the least, and that the vast discrepancy between our measures arose from the existence of a double dispersive power. This conclusion was confirmed by determining the dispersive power of the greatest refraction, which coincided exactly with the order assigned to it by Dr. Wollaston.

The dispersive powers, which I have obtained for other doubly refracting crystals, such as carbonate of strontites, carbonate of lead, and chromate of lead, have confirmed this result, and establish the general law, that each refraction of crystals which give double images is accompanied with a separate dispersive power. The double dispersive powers of these bodies are given in the following table.

Chromate of lead	(gr. refr.)	estimated at	0.400
Ditto	Ditto	must exceed	0.296
Ditto	(least refr.)	- -	0.262
Carbonate of lead	(gr. refr.)	- +	0.091
Ditto	(least refr.)	- -	0.066
Carbonate of strontites	(gr. refr.)	-	0.046
Ditto	(least refr.)	-	0.027
Calcareous spar	(gr. refr.)	- -	0.040
Ditto	(least refr.)	-	0.026

In a table of refractive powers, published by the late Mr. Cavallo, he has given, from other authors, the *dispersions*, or the *dissipations* as he calls them, of a few substances, and he has annexed a different *dispersion* to the two refractions of

Iceland crystal; but it is obvious, from a simple inspection of the table, that these are measures of the *dispersion* or *quantity of colour*, and not of the *dispersive power* of the substances. The measures in the table alluded to, with the exception of one or two, are so completely incompatible with those taken by Dr. WOLLASTON and myself, that I can scarcely believe that the experiments were ever made.

The singular property of a double dispersive power, while it seems to exclude some of the theories by which the double refraction has been explained, adds another to those numerous difficulties with which philosophy has yet to struggle, before she can reduce to a satisfactory generalization those anomalous and capricious phenomena which light exhibits in its passage through transparent bodies.

I have the honour to be,

dear Sir,

your most obedient humble servant,

DAVID BREWSTER.

Edinburgh, 23, Duke-street,
December 19, 1812.

To Sir H. Davy, LL.D. F. R. S.

XIV. *An Appendix to Mr.* Ware's *Paper on Vision. By Sir* Charles Blagden, *F. R. S.*

Read February 4, 1813.

Mr. Ware states in his Paper, that near sightedness comes on most frequently at an early age; that it is more common in the higher than in the lower ranks of life; and that particularly at the universities, and various colleges, a large proportion of the students make use of concave glasses. All this is exactly true, and to be accounted for by one single circumstance; namely, the habit of looking at *near* objects. Children born with eyes which are capable of adjusting themselves to the most distant objects, gradually lose that power soon after they begin to read and write; those who are most addicted to study become near sighted more rapidly; and, if no means are used to counteract the habit, their eyes at length lose irrecoverably the faculty of being brought to the adjustment for parallel rays. Of this I am myself an example, and as I recollect distinctly the progress, it may not be useless to record it here.

When I first learned to read, at the usual age of four or five years, I could see most distinctly, across a wide church, the contents of a table on which the Lord's Prayer, and the Belief, were painted in suitably large letters. In a few years, that is, about the ninth or tenth of my age, being much addicted to books, I could no longer read what was painted on this table; but

the degree of near sightedness was then so small, that I found a watch-glass, though as a meniscus it made the rays diverge very little, sufficient to enable me to read the table as before. In a year or two more, the watch-glass would no longer serve my purpose; but being dissuaded from the use of a common concave glass, as likely to injure my sight, I suffered the inconvenience of a small degree of myopy, till I was more than thirty years of age. That inconvenience, however, gradually though slowly increasing all the time, at length became so grievous, that at two or three and thirty, I determined to try a concave glass; and then found, that the numbers 2 and 3 were to me in the relation so well described by Mr. WARE; that is, I could see distant objects tolerably well with the former number, but still more accurately with the latter. After contenting myself a little time with N°. 2, I laid it wholly aside for N°. 3; and, in the course of a few more years, came to N°. 5, at which point my eye has now been stationary between fifteen and twenty years. An earlier use of concave glasses would probably have made me more near sighted, or would have brought on my present degree of myopy at an earlier period of life. If my friends had persuaded me to read and write with the book or paper always as far from my eye as I could see; or if I had occasionally intermitted study, and taken to field sports, or any employment which would have obliged me to look much at distant objects, it is very probable that I might not have been near sighted at all. Possibly the persons who become near sighted by having constantly to adjust their eyes to near objects, may not usually change to be long sighted by age.

On the subject of vision, I may be allowed to take this

opportunity of relating an experiment made many years ago, to decide how far the *similarity* of the images seen by each eye contributed to make them impress the mind as *one*. In the house where I then lived was a marble chimney-piece, the upper horizontal block of which was fluted vertically; and the ridge between each concavity of the fluting was about as wide as the concavity itself. When I looked at this range of fluting at the distance of about nine inches, and directed the optic axes to it, I saw of course every ridge and concavity distinctly, and judged rightly of the distance. Adjusting the optic axes as to an object a little further off, I discerned the fluting confusedly and all double, the ridges interfering with the concavities; which was accompanied with the uneasy sensation of squinting. But on widening the direction of the optic axes still more, as to an object about eighteen inches distant; (namely, just so far that the duplication of the images should correspond successively; that is, so that the first ridge and concavity of the fluting, as seen by one eye, should fall in with the second ridge and concavity, as seen by the other;) the fluting appeared as distinct and as single as at first; but it seemed to be about double the distance from the eye that it really was, and to be magnified in proportion; nor had I, in this case, any sensation of squinting. As the parts of the fluting, though in general much alike, were not exactly so every where in colour and minute circumstances, there appeared in some places a slight confusion from this dissimilarity of the images; but that trifling confusion had no manner of effect on the mind's judgment of the images, which looked as perfectly single, as when the fluting was viewed with the optic axes so directed, that the ridges and concavities seen by

one eye corresponded with the same ridges and concavities as perceived by the other. No idea was suggested, but that of a range of fluting larger and more distant than it was in fact. This experiment I frequently repeated, and always with the same effects.

XV. *A Method of drawing extremely fine Wires.* *By* William Hyde Wollaston, *M. D. Sec. R. S.*

Read February 18, 1813.

It is recorded by Musschenbroek, that an artist of Augsburg drew a wire of gold so slender, that five hundred feet of it weighed only one grain; but the method by which this was effected is not mentioned, and indeed it has been doubted, whether it could really have been done. I shall however shew, that a wire of gold may, without much difficulty, be obtained finer than that spoken of by Musschenbroek, and that wires of platina may be drawn much more slender, with the utmost facility.

Those who draw silver wire in large quantities for lace and embroidery, sometimes begin with a rod that is about three inches in diameter, and ultimately obtain wires that are as small as $\frac{1}{500}$ of an inch in thickness. If in any stage of this process a rod of silver wire be taken, and a hole be drilled through it longitudinally, having its diameter one-tenth part of that of the rod, and if a wire of pure gold be inserted, so as to fill the hole, it is evident that by continuing to draw the rod, the gold within it will be reduced in diameter exactly in the same proportion as the silver; so that if both be thus drawn out together till the diameter of the silver is $\frac{1}{500}$ of an inch, then that of the gold will be only $\frac{1}{5000}$; and of such wire five hundred and fifty feet would be requisite to weigh one grain.

For the purpose of removing the coating of silver that surrounds it, the wire must be steeped for a few minutes in warm nitrous acid, which dissolves the silver without danger of doing any injury to the gold. And though it might be difficult in this manner to preserve any considerable length of such wires, it is of little importance for any of those uses to which it is likely to be applied.

In my endeavours to make slender gold wires by the method above described, the difficulty of drilling the central hole in a metal so tough as fine silver, was greater than I had expected, and I was induced to try whether platina might not be substituted for the gold, as in that case its infusibility would allow me to coat it with silver without the necessity of drilling.

Having formed a cylindrical mould $\frac{1}{5}$ of an inch in diameter, I fixed in the centre of it a platina wire previously drawn to the $\frac{1}{100}$ of an inch, and then filled the mould with silver. When this rod was drawn to $\frac{1}{30}$, my platina was reduced to $\frac{1}{1000}$, and by successive reduction I obtained wires of $\frac{1}{4000}$ and $\frac{1}{5000}$, each excellent for applying to the eye-pieces of astronomical instruments, and perhaps as fine as can be useful for such purposes.*

Since this had been the primary object that I had in view, I should have thought my time ill bestowed in pursuing farther the practical application of a method to which there seems no limit, except the imperfections of the metal employed. But as I found by trial the tenacity of these wires to be greater than was to be expected in proportion to their substance, that

* No very accurate observations can be made with a telescope shorter than thirty inches, and at that distance $\frac{1}{4500}$ of an inch subtends only one second of a degree.

circumstance excited some doubts regarding the correctness of the estimate by which their diameter had been deduced. Other wires were consequently drawn with the utmost care, as to the quality and substance of the platina employed, and as to the proportional reduction of its diameter in the process of wire-drawing.

The extremity of a platina wire having been fused* into a globule nearly $\frac{1}{4}$ of an inch in diameter, was next hammered out into a square rod, and then drawn again into a wire $\frac{1}{253}$ of an inch in diameter. One inch of this wire duly coated with silver was drawn till its length was extended to 182 inches, consequently the proportional diminution of the diameter of the platina will be expressed by the square root of 182, so that its measure had become $\frac{1}{253 \times 13,5} = \frac{1}{3425}$. The specific gravity of the coated wire was assumed to be 10,5, and since the weight of 100 inches was 114 grains, its diameter was inferred to be $\frac{1}{42,8}$ of an inch, or just eighty times that of the platina contained in it.

With portions of the platina wire thus obtained, and successively reduced in diameter, I had an opportunity of repeating the trials of its tenacity with greater confidence in the justness of the estimate, and the results shewed generally (though with some accidental exceptions) that the process of wire-drawing, which is well known to improve the strength of

* I am indebted to my friend Dr. MARCET for the simple and easy method by which the fusion was effected. A piece of wire, about six inches long, having been bent to an angle in the middle, one half of its length was held in the flame of a spirit lamp impelled by a current of oxygen, and its extremity was thus fused in about half a minute.

metals within moderate limits, continued also to add something to the tenacity of platina even as far as $\frac{1}{18,000}$ of an inch, which supported $1\frac{1}{3}$ grain before it broke; but the wire on which these experiments were made began then to be impaired by repetition of the operation: so that although I afterwards obtained portions of it, as small as $\frac{1}{30,000}$ of an inch in diameter, it was in many places interrupted, and I could place no reliance upon any trials of its tenacity.

There are some little circumstances in the management of these fine wires, which it may be of advantage to describe for the assistance of those who would apply them to any useful purpose. When the diameter is not less than $\frac{1}{2000}$ or $\frac{1}{3000}$ of an inch, the difficulty of seeing and applying them in short pieces is not considerable; but when their diameter is farther reduced, and their length as much as an inch or more, the slightest current of air is sufficient to defeat all attempts to lay hold of an object so difficult to see, and so impossible to feel. It is therefore necessary to retain a part of the silver coating at each extremity, which, at the same time that it assists in finding the end, also serves to stretch the wire with a certain moderate tension, and affords the means of attaching it in any required position.

The method that I have found most convenient is to bend a portion of the coated wire into the shape of the letter U, with small hooks at its upper extremities. In this form it will conveniently hang upon a wire of gold or of platina, with the lowest part immersed in nitrous acid, till the coating of silver is removed from that part. It may then, without difficulty, be

lifted from its place, by one of the hooks alone, to any other situation, or suspended by it, with the other hook downwards, as the means of attaching a small chain, or other series of equal weights in trials of its tenacity.

XVI. *Description of a single-lens Micrometer. By* William Hyde Wollaston, *M. D. Sec. R. S.*

Read February 25, 1813.

Having had occasion to measure some very small wires with a greater degree of accuracy than I was enabled to do by any instrument hitherto made use of for such purposes, I was led to contrive other means that might more effectually answer the end proposed. The instrument to which I had recourse is furnished with a single lens of about $\frac{1}{12}$ of an inch focal length. The aperture of such a lens is necessarily small, so that when it is mounted in a plate of brass, a small perforation can be made by the side of it in the brass as near to its centre as $\frac{1}{25}$ of an inch.

When a lens thus mounted is placed before the eye for the purpose of examining any small object, the pupil is of sufficient magnitude for seeing distant objects at the same time through the adjacent perforation, so that the apparent dimensions of the magnified image might be compared with a scale of inches, feet, or yards, according to the distance at which it might be convenient to place it. A scale of smaller dimensions attached to the instrument will, however, be found preferable on account of the steadiness with which the comparison may be made; and it may be seen with sufficient distinctness by the naked eye, without any effort of nice adaptation, by reason of the smallness of the hole through which it is viewed.

The construction that I have chosen for the scale is represented in fig. 1. (See Plate VI.) It is composed of small wires, about $\frac{1}{50}$ of an inch in diameter, placed side by side, so as to form a scale of equal parts, which may with ease be counted by means of a certain regular variation of the lengths of the wires.

The external appearance of the whole instrument is that of a common telescope, consisting of three tubes. The scale occupies the place of the object glass, and the little lens is situated at the smaller end, with a pair of plain glasses sliding before it, between which the subject of examination is to be included. This part of the apparatus is shewn separately in fig. 3. It has a projection at *a*, with a perforation through which a pin is inserted to connect it with a screw represented at *b*, fig. 2. This screw gives lateral motion to the object, so as to make it correspond with any particular part of the scale. The lens has also a small motion of adjustment by means of the cap *c*, fig. 2, which renders the view of the magnified object distinct.

Before the instrument is completed, it is necessary to determine with precision the indications of the scale, which must be different according to the distance to which the tube is drawn out. In my instrument, one division of the scale corresponds to $\frac{1}{10000}$ of an inch when it is at the distance of 16,6 inches from the lens; and since the apparent magnitude in small angles varies in the simple inverse ratio of this distance, each division of the same scale will correspond to $\frac{1}{5000}$ at the distance of $8\frac{3}{10}$ inches, and the intermediate fractions $\frac{1}{6000}$, $\frac{1}{7000}$, &c. are found by intervals of 1,66 inch marked on the outside of the tube. The basis on which these indications were founded in this instrument, was a wire carefully ascertained

to be $\frac{1}{200}$ of an inch in diameter, the magnified image of which occupied fifty divisions of the scale, when it was at the distance of 16,6 inches, and hence one division $= \frac{1}{50 \times 200} = \frac{1}{10000}$. Since any error in the original estimate of this wire must pervade all subsequent measures derived from it, the substance employed was pure gold drawn till fifty-two inches in length weighed exactly five grains. If we assume the specific gravity of gold to be 19,36, a cylindrical inch will weigh 3837 grains, and we may thence infer the diameter of such a wire to be $\frac{1}{200}$ of an inch, more nearly than can be ascertained by any other method. For the sake of rendering the scale more accurate, a similar method was in fact pursued with several gold wires, of different sizes, weighed with equal care; and the subdivisions of the exterior scale were made to correspond with the average of their indications.

In making use of this micrometer for taking the measure of any object, it would be sufficient at any one accidental position of the tube to note the number on the outside as denominator, and to observe the number of divisions and decimal parts which the subject of examination occupies, on the interior scale, as numerator of a fraction expressing its dimensions in proportional parts of an inch; but it is preferable to obtain an integer as numerator, by sliding the tube inward or outward, till the image of the wire is seen to correspond with some exact number of divisions, not only for the sake of greater simplicity in the arithmetical computation, but because we can by the eye judge more correctly of actual coincidence, than of the comparative magnitudes of adjacent intervals.

The smallest quantity, which the graduations of this instru-

ment profess to measure, is less than the eye can really appreciate in sliding the tube inward or outward. If, for instance, the object measured be really $\frac{1}{9900}$, it may appear $\frac{1}{10000}$ or $\frac{1}{9800}$, in which case the doubt amounts to $\frac{1}{50}$ part of the whole quantity. But the difference is here exceedingly small in comparison to the extreme division of other instruments where the nominal extent of its power is the same. A micrometer with a divided eye-glass may profess to measure as far as $\frac{1}{10000}$ of an inch; but the next division is $\frac{2}{10000}$ or $\frac{1}{5000}$; and, though the eye may be able to distinguish that the truth lies between the two, it receives no assistance within $\frac{1}{2}$ part of the larger measure.

Fig 3

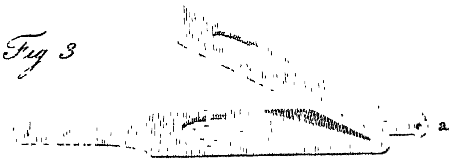
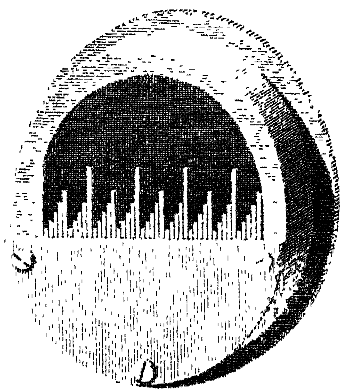


Fig. 1.



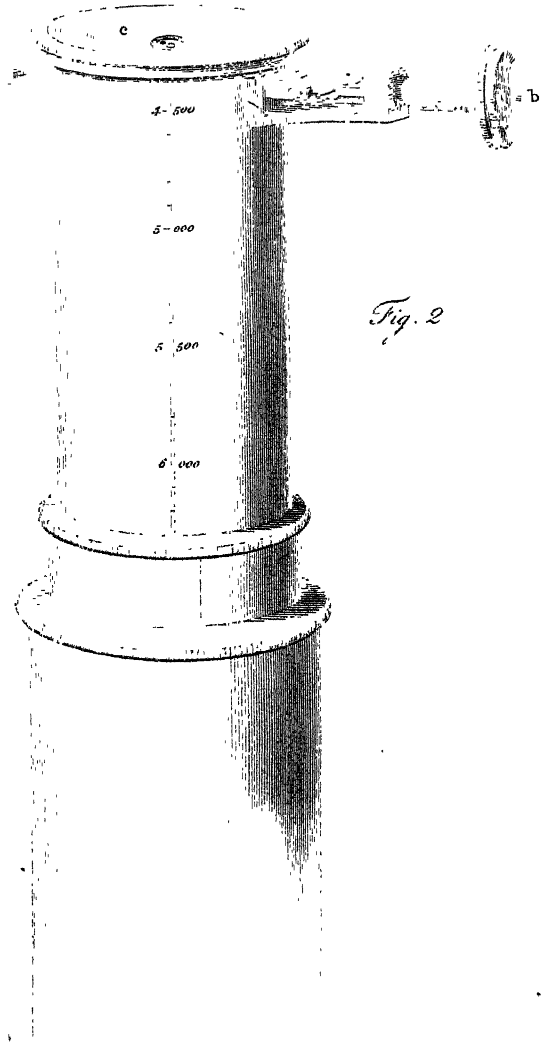


Fig. 2

J. Basire sc.

XVII. *Observation of the Winter Solstice of* 1812, *with the Mural Circle at Greenwich. By* John Pond, *Esq. Astronomer Royal, F.R.S.*

Read February 25, 1813.

THE weather this year at the period of the solstice was peculiarly unfavourable for astronomical observation; however, in the course of the month, I obtained nine observations of the sun; one of these proved defective, the result of the other eight, accompanies this communication. In my observation of the summer solstice, it will be seen that I assumed the arc ZP equal 38° 31′ 21″15; by subsequent observation I conceive that I have somewhat improved this quantity, which I now assume 38° 31′ 21″,5, as resulting from 120 observations of Polaris.

The observation of the summer solstice thus corrected will give the mean obliquity of the ecliptic for January 1, 1813, 23° 27′ 51″,50, and the winter solstice 23° 27′ 47″,35.

There can be no doubt but this small discordance might easily be reconciled by a slight modification of Bradley's refractions, and perhaps ultimately it may be necessary to have recourse to this theory for its explanation; but I am unwilling to do this hastily, being now occupied in making an extensive series of observations of circumpolar stars, with a view of determining, if possible, whether Bradley's mean refraction does, or does not, require alteration.—As I propose making the discordance of the solstices the subject of a sepa-

rate paper, I shall not add any thing farther on the subject, excepting a recommendation to astronomers, to deduce their refractions from circumpolar stars altogether, and then examine the solstices with those refractions, and by no means to make the coincidence of the solstices a required proof of their accuracy.

Note. In the annexed computation the reduction to the solstice is computed from the *observed* right ascension of the sun, by this means the errors of the tables being avoided, observations at a considerable distance (even some weeks from the solstice) may be employed, particularly in the winter season, when the uncertainty from refraction is greater than the error which may be probably introduced into this part of the calculation.

1812.	Barometer.	Therm. In.	Therm. Out.	Refraction.	Observations as given by the Instrument.	Position of the Zero point, or Equation to be applied to obtain the N. P. D.	Equation to reduce the observed N. P. D. to Zenith Distance or position of the Zenith point on the Circle.	Semidiameter of the ⊙ 1" greater than in the Nautical Almanack.	Reduction to the Solstice.	Solstitial Zenith Distance.	N. P. D.
					° ′ ″	′ ″	° ′ ″	′ ″	′ ″	° ′ ″	° ′ ″
Dec. 6	30,19	41	38	3 21,1	⊙ UL 112 12 34,3	+ 0 6,5	38 31 15,0	+ 16 17,3	55 31,3	74 56 29,0	113 27 50,5
7	30,42	34	29	3 35,2	LL 112 51 44,4	6,5	15,0	— 16 17,3	48 37,4	24,7	46,2
9	29,81	29	24	3 36,5	LL 113 4 14,8	6,5	15,0	— 16 17,5	36 10,0	28,8	50,3
10	29,73	33	30	3 27,6	UL 112 37 19,5	6,5	15,0	+ 16 17,6	30 36,8	26,5	48,0
11	29,84	35	34	3 36,0	LL 113 14 49,1	6,5	15,0	— 16 17,7	25 30,3	22,7	44,2
13	29,61	32	30	3 30,6	UL 112 51 13,0	6,5	15,0	+ 16 18,0	16 41,8	28,4	49,9
15	29,44	32	31	3 39,3	LL 113 30 39,6	6,5	15,0	— 16 18,1	9 43,4	29,2	50,7
30	30,10	45	47	3 26,8	UL 112 50 38,8	6,5	15,0	+ 16 18,8	17 18,0	27,4	48,9
					Mean of 8 Observations - - - - -					74 56 27,09	113 27 48,59
					Nutation + 7″,62 Parallax — 8″,50 ⊙ Lat. — 0″,36 =					— 1,24	— 1,24
										74 56 25,85	113 27 47,35

XVIII. *On the Tusks of the Narwhale. By* Sir Everard Home, *Bart. F.R.S.*

Read February 18, 1813.

THE structure of many animals that inhabit the great Northern Ocean, is, even at this day, imperfectly known; this arises from those who have the best opportunities of making such enquiries not being fitted for them, or being too much engaged in pursuits of a different nature. Under such circumstances too much praise cannot be bestowed on the few individuals, whose zeal for science induces them to exert themselves in improving this branch of knowledge; to one of these, Mr. SCORESBY, jun. I am indebted for the means of making the following observations on the tusks of the narwhale.

Mr. SCORESBY told me, a year ago, that the female narwhale had no tusks, which astonished me; and the only reply I could make to such an assertion, was to beg that he would procure me a skull, that I might be satisfied of the fact. This he promised to do, and last summer sent me the skull of a female, in which there was no appearance whatever of tusks; and as the sutures were all united, there was every reason to believe the time of having teeth had elapsed, particularly as a male skull of the same size, and in which the sutures were not equally well united, had a tusk four feet long.

With such evidence before me, I was naturally led to adopt

the generally received opinion of the captains in the Greenland fishery, that the males had one tusk, and the females none; and as I imagined that I had cleared up a part of the natural history of this species of whale, which had hitherto been involved in obscurity, I proceeded to lay these observations before the Society. After I had done so, I found so many contradictory accounts among my friends, that I became staggered what to believe: some had seen two tusks of different lengths in the same skull, others believed they had seen two of the same length. To set the question at rest, my friend Mr. BROWN, Librarian to Sir JOSEPH BANKS, took the trouble of collecting all the books in Sir JOSEPH's library, in which the subject is mentioned. In ANDERSON's Description of Iceland, Greenland, and Davis's Straits, it is mentioned that, in 1684, DICK PETERSON brought to Hamburgh the skull of a female narwhale with two tusks, the left seven feet five inches, the right seven feet long, and that this skull had ever since been preserved there, and shewn to the curious. This account is copied by several later authors.

I found also, in TYCHO L. TYCHONIUS, an account, published in 1706, of a narwhale's skull with the left tusk seven feet long, and the right imbedded and completely concealed in the substance of the skull, nine Danish inches in length. The author takes some merit, and in my opinion deservedly, for having discovered it. A drawing of it *in situ* is annexed.

This information set me to work in sawing the skulls in the HUNTERIAN collection, to ascertain whether they contained rudiments of tusks not yet protruded from the substance of the bone, and the result of this investigation explains, in the most satisfactory manner, every thing that I have seen

written, or heard asserted upon this subject, and clears up any apparent difference between them.

In one male skull, in which the sutures were tolerably firmly united, the tusk in the left side is seven feet nine inches long; a small tusk, nine inches long, is imbedded in the bone on the right side, with a bulb or swelling at its root, and the point six inches from the front of the skull, which is quite solid, and has no external orifice.

In another male skull, which must have belonged to a younger animal, since the sutures are not completely united, the left tusk is four feet long, and the right one, concealed within the skull, is nine inches and three quarters long, and its point seven and a quarter distant from the front of the skull. In this specimen there is an external orifice leading down to the point of the young tusk, so that in this respect the small tusk is more advanced than in the older one, shewing that there is a great variety in the time of the second tusk coming forward.

The young tusks have not the spiral turns upon them, but are ribbed on the surface, and the ridges have a tendency to the left side. These are milk tusks, since they have come to their full growth, and are quite solid throughout their whole length, similar to the milk tusk of the elephant, which is however only two inches long before it is shed.

Upon sawing a full grown tusk in a longitudinal direction, I found that, contrary to what happens in the tusks of other animals, there is a hollow tube in the middle through the greater part of its length, the point, and the portion at the root, only being solid.

From these two specimens, there can be no doubt that the

left tusk appears commonly long before the right one, which corresponds with the accounts given by the captains employed in the Greenland fishery. One of these captains, who has been thirty-five voyages, informed me that he never saw a male narwhale with two tusks, except once from the mast head; the animal was rising out of the water, the left tusk was about six feet above the surface, and the point of the right tusk just out of the water, so that it appeared to him one-third the length of the left.

In the skull of the female sent me by Mr. SCORESBY, the sutures are more united than in the smallest of the males which I have described; there is no appearance whatever of tusks externally, but both on the right and left side there is an orifice in the bone, and when the skull was cut into, two small milk tusks were discovered of the same size and appearance, and exactly resembling those described in the male; they were eight inches long, and the points were only two inches and a quarter from the front of the skull, lying in a canal, of which the external opening was the orifice, so that they were nearer getting into the gum than those of either of the males; and there can be no doubt that the permanent tusks, which were to follow them, would be of equal lengths, or nearly so throughout their growth, as they were found to be in the skull at Hamburgh. We learn also, from this specimen, that the tusks in the female come much later than in the male, which explains the error the captains of the Greenland ships have been led into, of the females having no tusks.

Female skulls, with full grown tusks, must be rarely met with, since the only well authenticated account upon record

of a skull with two tusks of equal lengths, is that given by DICK PETERSON.

These facts make it necessary to take from this species of whale the name given to it by LINNÆUS, of Monodon Monoceros, since they prove that it is a very improper one.

The greatest length which has been given to the left tusk, before the right has cut the gum, is fifteen feet: this account is mentioned by EGEDE in his Natural History of Greenland, 1741.

The lower jaw, both in the male and female, has a rounded edge, in which there is no part from which teeth can grow.

EXPLANATION OF THE PLATE.

(See Plate VII.)

Fig. 1. The young skull of a male narwhale, shewing the permanent tusk in its socket, the milk tusk ready to be protruded.

Fig. 2. The female skull, with the two milk tusks ready to be protruded, having acquired their full size, and canals being formed through which the points are to pass out.

Fig. 3. A section of a milk tusk to shew that it is solid.

Fig. 4. The lower jaw, in which there is no place for teeth.

Fig. 5. A section of a full grown tusk, to shew the cavity in the middle, and that the parts at the point and at the root are solid.

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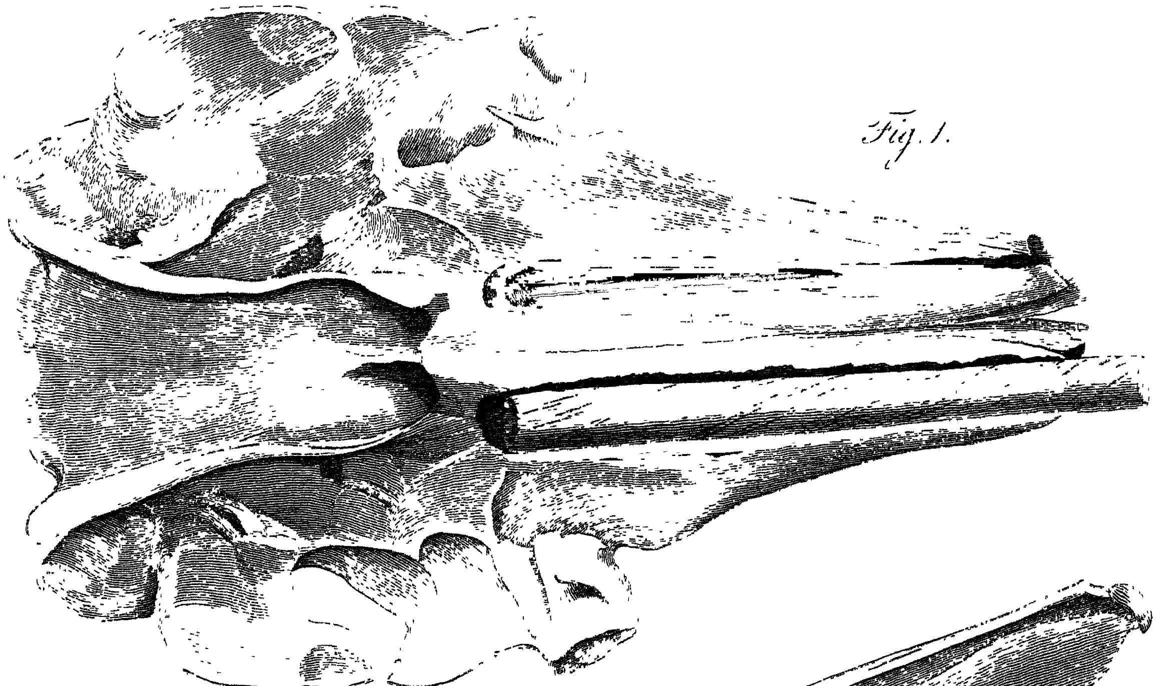


Fig. 1.

3 Inches to a Foot

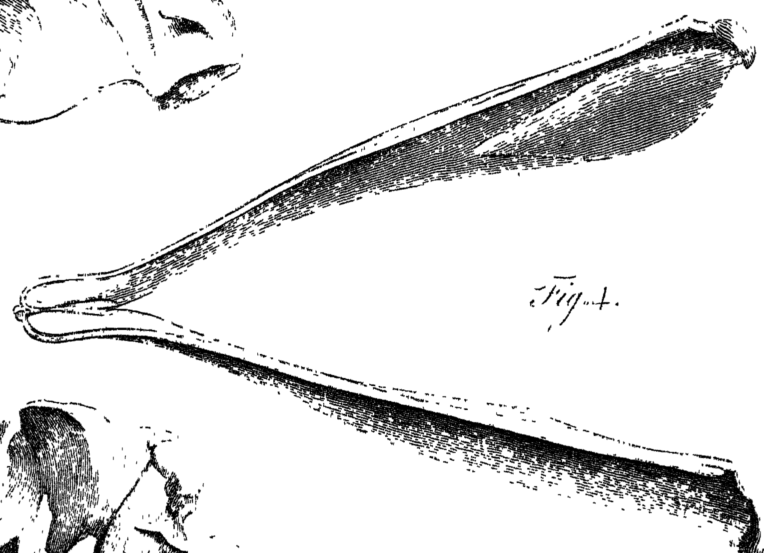


Fig. 4.

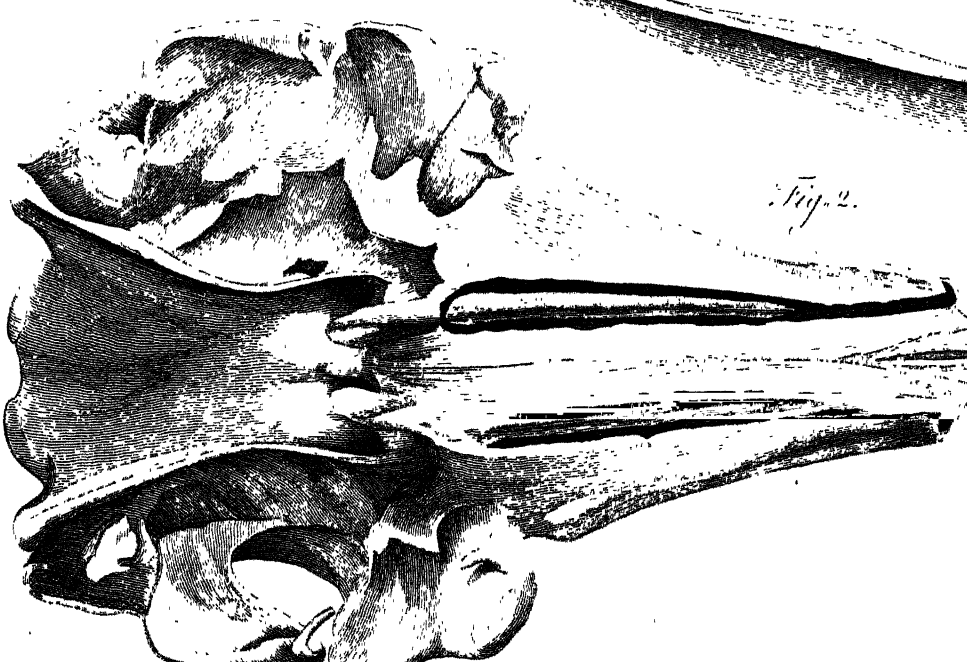
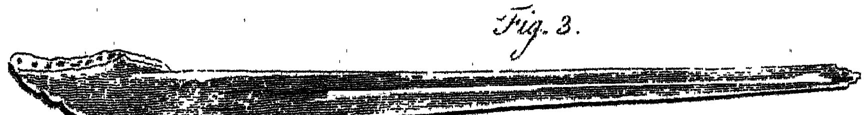


Fig. 2.

Fig. 5.

1 Inch & 3/4 to a Foot.

Fig. 3.



6 Inches to a Foot.

J. Basire sc.

METEOROLOGICAL JOURNAL,

KEPT AT THE APARTMENTS

OF THE

ROYAL SOCIETY,

BY ORDER OF THE

PRESIDENT AND COUNCIL.

METEOROLOGICAL JOURNAL

for January, 1812.

1812	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Hygrometer. °		Winds. Points.	Winds. Str.	Weather.
Jan. 1	9	0	36	43	29,85	73	Rain this Month 0,779 Inches.	W	1	Hazy.
	3	0	39	46	29,78	71		W	1	Fine.
2	9	0	38	44	29,63	75		W	1	Fine.
	3	0	43	47	29,56	72		SSW	1	Fine.
3	9	0	37	45	29,44	70		SSE	1	Cloudy.
	3	0	41	44	29,32	70		S	1	Rain.
4	9	0	34	45	29,48	71		SW	1	Thick and Cloudy.
	3	0	39	47	29,44	70		E	1	Cloudy.
5	9	0	34	45	29,23	75		N	1	Snow.
	2	0	36	45	29,18	75		NE	1	Snow.
6	9	0	32	44	29,74	71		W	1	Hazy.
	3	0	43	47	29,75	69		W	1	Cloudy.
7	9	0	33	45	29,63	71		W	1	Snow, much. Snow in the night.
	3	0	38	43	29,74	72		N	2	Cloudy.
8	9	0	34	45	30,08	71		N	1	Fine.
	3	0	37	47	30,16	70		NNE	1.2	Cloudy.
9	9	0	30	44	30,28	70		N	1	Foggy.
	3	0	36	47	30,21	67		W	1	Cloudy.
10	9	0	34	46	30,66	74		W	1	Cloudy.
	3	0	37	48	30,19	74		NNE	1	Foggy. Snow in the night.
11	9	0	35	47	30,15	74		W	1	Thick fog.
	3	0	38	47	29,99	71		W	1	Cloudy.
12	9	0	35	45	29,92	70		NNE	1	Hazy.
	3	0	39	45	29,90	70		N	1	Fine.
13	9	0	34	44	29,74	72		W	1	Thick and hazy.
	4	0	39	47	29,73	70		N	1	Cloudy.
14	9	0	37	47	30,02	72		W	1	Cloudy.
	3	0	40	49	30,10	69		W	1	Cloudy.
15	9	0	37	46	30,13	72		W	1	Cloudy.
	3	0	43	49	30,13	70		W	1	Hazy.
16	9	0	37	46	30,24	72			1	Foggy.
	3	0	38	49	30,18	72		E	1	Cloudy.

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for January, 1812.

1812	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Hygrometer. °		Winds. Points.	Winds. Str.	Weat[her]
Jan. 17	9	0	37	47	30,25	73	Rain this Month 0,779 Inches.	W	1	Cloudy.
	3	0	40	50	30,23	68		WNW	1	Cloudy.
18	9	0	35	48	30,26	69		W	1	Cloudy.
	3	0	44	49	30,24	71		W	1	Cloudy.
19	9	0	42	48	30,18	74		W	1	Rain.
	3	0	47	48	30,04	71		W	1.2	Cloudy.
20	8	0	38	47	29,97	68		NW	1	Fine.
	4	0	39	50	29,95	61		NNW	1	Fine.
21	8	0	32	47	29,84	65		W	1	Fair.
	4	0	36	51	29,83	61		W	1	Fine.
22	8	0	35	47	29,83	67		NW	1	Cloudy.
	3	0	38	51	29,82	67		N	1	Cloudy.
23	9	0	33	47	29,93	65		N	1	Cloudy.
	4	0	35	49	30,02	64		NE	1	Cloudy.
24	9	0	33	47	30,13	66		NW	1	Cloudy.
	3	0	38	50	30,12	63		NW	1	Fine, but rathe
25	8	0	32	46	30,08	68		W	1	Cloudy.
	4	0	40	48	30,04	67		W	1	Cloudy.
26	9	0	41	47	30,00	75		SW	1	Rain.
	9	0	38	47	30,09	71		W	1	Fine.
27	9	0	41	46	30,03	75		SW	1	Rain.
	3	0	45	49	29,91	73		SW	1	Cloudy.
28	8	0	44	48	29,83	71		SSW	1	Cloudy.
	3	0	44	50	29,70	70		SW	1	Cloudy.
29	9	0	39	47	29,33	69		E	1	Cloudy.
	3	0	43	50	29,20	63		S	1.2	Cloudy.
30	9	0	42	48	29,25	73		S	1	Fine.
	4	0	45	52	29,32	72		S	1	Fine.
31	9	0	38	49	29,78	74		S	1	Fine.
	3	0	45	53	29,79	71		SE	1	Cloudy.

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for February, 1812.

1812	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Hy-gro-me-ter. °		Winds. Points.	Winds. Str.	Weather.
Feb. 1	9	0	41	51	29,77	74	Rain this Month 1,702 Inches.	E	1	Rain.
	3	0	45	52	29,64	71		SE	1	Rain.
2	9	0	44	50	29,66	72		SSE	1	Fine.
	4	0	46	51	29,46	72		SE	1	Rain. [gale of wind in the night.
3	8	0	42	50	29,58	73		SE	1	Fine and clear. A very heavy
	3	0	47	52	29,44	72		E	1	Cloudy.
4	9	0	46	51	29,36	75		ESE	1	Cloudy.
	3	0	48	54	29,44	74		S	1	Rain.
5	9	0	45	52	29,55	75		SE	1	Cloudy.
	3	0	46	53	29,50	75		E	1	Rain.
6	9	0	45	52	29,44	77		E	1	Thick and cloudy.
	4	0	45	53	29,47	75		NNE	1	Cloudy.
7	9	0	37	52	29,91	67		W	1	Fair.
	3	0	47	56	29,88	64		W	1	Cloudy.
8	8	0	39	52	29,69	70		W	1	Cloudy.
	4	0	43	53	29,75	67		NW	1	Rain.
9	9	0	40	52	29,99	68		S	1	Cloudy.
	4	0	41	51	29,99	64		S	1	Cloudy.
10	8	0	40	50	29,93	69		E	1	Cloudy and foggy.
	4	0	42	54	29,87	64		E	1	Fine.
11	8	0	35	50	29,85	71		E	1	Foggy.
	4	0	42	55	29,77	64		S	1	Fine.
12	8	0	46	51	29,54	75		SSW	2	Rain.
	4	0	48	53	29,42	74		S	2	Rain.
13	8	0	40	51	29,44	71		W	1	Fine.
	4	0	45	54	29,67	60		W	1	Fine.
14	8	0	43	51	29,54	73		SW	2	Rain.
	4	0	47	54	29,33	68		W	1	Cloudy.
15	8	0	41	51	29,63	67		W	1	Cloudy.
	3	0	45	55	29,68	60		WNW	1	Fine.
16	9	0	44	52	29,66	69		W	1	Cloudy.
	3	0	48	52	29,69	65		W	1	Cloudy.

METEOROLOGICAL JOURNAL
for February, 1812.

1812	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Hygrometer. °		Winds. Points.	Winds. Str.	Weather.
Feb. 17	8	0	48	52	29,59	76	Rain this Month 1,702 Inches.	W	1	Rain.
	4	0	50	54	29,40	71		SW	1	Rain.
18	8	0	42	53	29,81	68		W	1	Cloudy.
	4	0	49	55	29,95	70		W	1	Cloudy.
19	9	0	45	53	30,12	72		S	1	Cloudy.
	4	0	48	55	30,11	68		SW	1	Cloudy.
20	8	0	40	52	29,99	71		E	1	Fair.
	4	0	47	57	29,89	68		S	1	Cloudy.
21	8	0	47	54	29,77	74		S	1	Cloudy.
	4	0	53	57	29,59	67		S	3	Cloudy and squally.
22	9	0	44	53	29,48	72		S	3	Rain.
	2	0	48	54	29,48	69		SW	2	Cloudy.
23	9	0	47	52	29,58	67		W	1	Cloudy.
	5	0	48	54	29,74	63		W	1	Cloudy.
24	9	0	38	52	29,49	73		NNE	2	Rain. Snow in the night.
	4	0	44	56	29,84	65		W	1	Cloudy.
25	8	0	40	52	29,75	70		S	1	Cloudy.
	4	0	42	54	29,20	72		SSW	3	Rain.
26	9	0	46	51	29,37	72		W	1	Fair.
	4	0	42	52	29,19	69		W	1	Cloudy.
27	9	0	37	50	29,78	71		S	1	Cloudy.
	4	0	43	51	29,70	71		S	1	Rain.
28	9	0	37	50	29,68	72		W	1	Fair.
	4	0	43	54	29,64	65		SW	1	Fine.
29	9	0	35	50	29,57	72		E	1	Hazy.
	4	0	44	53	29,49	66		E	1	Cloudy.

METEOROLOGICAL JOURNAL

for March, 1812.

1812	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Hygrometer.		Winds. Points.	Winds. Str.	Weather.
Mar. 1	9	0	41	50	29,46	70°	Rain this Month 1,870 Inches.	E	1	Cloudy.
	4	0	43	50	29,50	64		E	1	Fair.
2	8	0	37	48	29,85	69		N	1	Fine.
	4	0	40	52	29,93	63		N	1	Fine.
3	9	0	35	48	29,93	67		E	1	Cloudy.
	4	0	41	50	29,88	70		S	1	Rain.
4	9	0	42	48	29,82	74		SW	1	Rain.
	4	0	50	51	29,77	76		W	1	Rain.
5	9	0	40	49	29,89	66		W	1	Fair.
	5	0	44	52	30,04	59		W	1	Fair.
6	8	0	40	50	29,70	73		WSW	1,2	Rain.
	4	0	49	52	29,79	60		W	1	Cloudy.
7	8	0	48	51	29,79	71		W	1	Cloudy.
	4	0	52	53	29,68	65		W	1	Cloudy.
8	8	0	41	51	29,89	69		W	1	Cloudy.
	4	0	44	51	29,95	63		NW	1	Cloudy.
9	9	0	38	49	30,26	65		N	1	Fair.
	4	0	41	54	30,28	65		N by E	1	Fine.
10	8	0	38	54	30,33	68		N	1	Cloudy.
	4	0	43	55	30,28	59		N	1	Fine.
11	8	0	36	49	30,27	71		N	1	Cloudy.
	4	0	42	51	30,27	61		E	1	Cloudy.
12	9	0	39	49	30,22	63		NE	1	Cloudy.
	4	0	42	51	30,10	61		W	1	Cloudy.
13	9	0	39	55	30,06	66		N	1	Fine.
	4	0	43	54	29,93	62		N by E	2	Rain.
14	8	0	38	49	30,04	70		NE	1	Cloudy.
	3	0	42	50	30,03	68		NE	1	Cloudy.
15	9	0	35	48	29,84	69		N	1	Fine.
	4	0	40	50	29,73	61		NW	1	Fine.
16	8	0	34	46	29,69	63		N	1	Cloudy.
	9	0	32	48	29,69	62				Cloudy.

METEOROLOGICAL JOURNAL
for March, 1812.

1812	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Hygrometer.		Winds. Points.	Winds. Str.	Weather.
Mar. 17	8	0	33	46	29,73	62	Rain this Month 1,870 Inches.	N	1	Cloudy.
	4	0	36	50	29,76	60		N	1	Cloudy.
18	9	0	32	45	29,56	60		NW	1	Fine, rather hazy.
	4	0	35	45	29,40	60		E	1	Cloudy.
19	9	0	32	44	29,30	62		SW	1	Cloudy.
	11	0	33	48	29,31	66		W	1	Cloudy.
20	8	0	34	46	29,13	73		W	1	Rain. Snow in the night.
	5	0	42	49	28,94	74		W	1	Rain.
21	8	0	43	47	29,13	71		S	2	Rain.
	5	0	47	56	29,26	59		S	1	Fine.
22	9	0	44	49	29,49	71		W	1	Cloudy.
	4	0	47	50	29,63	68		E	1	Cloudy.
23	8	0	40	48	29,72	73		E	1	Cloudy.
	4	0	43	50	29,52	72		E	1	Cloudy.
24	9	0	41	49	29,14	76		NE	1	Cloudy.
	5	0	40	50	29,27	72		N	1	Rain
25	8	0	35	48	29,86	71		NNE	1,2	Cloudy. Snow in the night.
	5	0	37	57	29,89	59		N	1	Fine.
26	8	0	32	47	30,35	64		N	1	Fair.
	5	0	39	56	30,38	59		E	1	Fine.
27	8	0	38	48	30,19	67		E	1	Cloudy.
	4	0	44	50	29,92	60		SW	1	Cloudy.
28	8	0	49	49	29,50	75		SW	1	Cloudy.
	4	0	51	57	29,33	70		SW	1	Rain.
29	8	0	50	49	29,47	70		W	1	Cloudy.
	4	0	51	52	29,56	72		S	1	Rain.
30	8	0	51	51	29,38	76		W	1	Cloudy.
	4	0	55	54	29,46	70		W	2	Rain.
31	8	0	43	52	29,83	69		E	1	Cloudy.
	5	0	41	54	29,77	67		E	1	Cloudy.

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for April, 1812.

1812	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Hygrometer.	Rain	Winds. Points.	Winds. Str.	Weather.
April 1	8	0	46	52	29,57	74°	Rain this Month 1,060 Inches.	S	1	Rain.
	4	0	53	55	29,60	66		W	1	Cloudy.
2	8	0	50	53	29,64	73		W	1	Cloudy.
	6	0	52	55	29,57	73		W	1	Cloudy.
3	8	0	53	55	29,49	74		W	1	Cloudy.
	5	0	53	57	29,61	65		W	1	Cloudy.
4	8	0	47	55	29,64	69		S	1	Rain.
	4	0	52	57	29,82	67		S	1	Cloudy.
5	8	0	44	54	30,17	65		E	1	Cloudy.
	4	0	51	56	30,16	62		SW	1	Cloudy.
6	8	0	45	54	30,22	67		W	1	Hazy.
	4	0	49	57	30,20	59		SW	1	Cloudy.
7	8	0	45	54	30,00	62		SE	1	Cloudy.
	5	0	43	56	29,89	64		E	1	Rain.
8	7	0	40	54	30,06	70		E	1	Rain.
	5	0	42	57	30,14	56		E	1	Cloudy.
9	8	0	37	51	30,19	63		S	1	Fine.
	6	0	49	54	30,13	57		W	1	Cloudy.
10	8	0	38	52	30,04	56		E	1	Cloudy.
	6	0	43	53	30,00	57		E	1	Cloudy.
11	8	0	40	52	29,99	61		E	1	Cloudy.
	5	0	43	52	29,94	58		W	1	Cloudy.
12	8	0	41	51	29,90	61		W	1	Cloudy.
	5	0	46	53	29,91	55		N	1	Fine.
13	8	0	42	50	30,06	61		E	1	Cloudy.
	5	0	44	54	30,07	60		E	1	Fine.
14	8	0	41	50	30,04	63		E	1	Fair.
	6	0	44	55	29,96	62		E	1	Cloudy.
15	8	0	42	51	29,86	67		NE	1	Cloudy.
	5	0	46	58	29,79	58		E	1	Fine.
16	8	0	42	51	29,68	67		E	1	Cloudy.
	6	0	45	53	29,67	63		N	1	Cloudy.

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for April, 1812.

1812	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Hygrometer.		Winds. Points.	Winds. Str.	Weather.
Apr. 17	8	0	40	51	29,85	63°	Rain this Month 1,060 Inches.	N	1	Fair.
	6	0	45	53	30,00	56		N	1	Fine.
18	9	0	40	50	30,06	63		N	1	Fine.
	10	0	42	53	30,13	56		N	1	Cloudy.
19	8	0	41	51	30,14	62		N	1	Cloudy.
	5	0	46	52	30,12	57		E	1	Fine.
20	8	0	40	50	30,07	63		W	1	Hazy.
	6	0	49	55	30,07	54		N	1	Fine.
21	8	0	44	52	30,21	63		N	1	Fine.
	6	0	53	55	30,16	54		W	1	Cloudy.
22	9	0	45	52	30,12	58		N	1	Cloudy.
	9	0	44	54	30,02	60		N	1	Cloudy.
23	8	0	40	51	30,06	62		N by W	1	Fine.
	11	0	42	53	30,06	60		N	1	Cloudy.
24	8	0	42	52	30,04	63		N	1	Cloudy.
	6	0	45	56	29,99	59		N	1	Cloudy.
25	8	0	42	53	29,91	63		W	1	Fine.
	4	0	45	55	29,75	65		W	1	Cloudy.
26	9	0	42	53	29,63	71		SW	1	Rain.
	11	0	42	52	29,65	65		E	1	Fine.
27	8	0	42	51	29,73	70		E	1	Cloudy.
	7	0	48	54	29,72	66		E	1	Rain.
28	8	0	48	53	29,70	72		E	1	Cloudy.
	6	0	48	55	29,77	71		ENE	1	Rain.
29	8	0	48	54	29,88	70		E	1	Cloudy.
	8	0	48	55	29,88	69		E	1	Rain.
30	8	0	47	53	29,85	76		E	1	Rain.
	7	0	49	55	29.90	73		E	1	Rain.

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for May, 1812.

1812	Time. H. M.		Therm. without. °	Therm. within. °	Barom. Inches.	Hy-gro-me-ter. °		Winds. Points.	Str.	Weather.
May 1	8	0	48	55	30,11	72	Rain this Month 1,932 Inches.	W	1	Cloudy.
	6	0	52	58	30,09	64		N	1	Cloudy.
2	8	0	48	56	30,02	67		E	1	Cloudy.
	7	0	48	57	29,90	64		E	1	Fair.
3	9	0	47	56	29,78	68		N	1	Cloudy.
	5	0	53	56	29,74	60		NE	1	Cloudy
4	9	0	47	55	29,78	62		E	1	Fine.
	4	0	50	55	29,93	63		N	1	Cloudy.
5	8	0	49	55	29,93	65		NE	1	Cloudy.
	4	0	52	57	29 97	66		E	1	Cloudy.
6	8	0	48	56	30,07	67		E	1	Fair.
	6	0	54	59	30,05	61		E	1	Fine.
7	8	0	49	55	30,05	60		E	1	Cloudy.
	5	0	53	60	29,99	61		E	1	Fair.
8	8	0	53	57	29,92	63		E	1	Fair.
	5	0	66	63	29,85	52		SE	1	Fair.
9	8	0	63	61	29,81	60		S	1	Fair.
	6	0	60	63	29,76	62		SW	1	Cloudy.
10	8	0	57	61	29,88	62		W	1	Cloudy.
	11	0	54	60	29,88	71		W	1	Cloudy.
11	8	0	55	60	29,64	72		W	2	Cloudy.
	5	0	56	60	29,63	68		W	1	Cloudy.
12	8	0	54	60	29,62	65		W	1	Cloudy.
	6	0	57	61	29,58	61		S	1	Rain.
13	8	0	51	59	29,60	63		SSW	1	Cloudy. [at 2 PM.
	7	0	53	60	29,57	63		W	1	Fair. Thunder and hail-storm
14	8	0	51	58	29,64	65		W	1	Fine.
	10	0	52	60	29,75	62		W	1	Fine.
15	8	0	52	58	29,83	62		N	1	Cloudy.
	6	0	55	59	29,88	61		NE	1	Cloudy.
16	8	0	48	57	30,04	70		NE	1	Cloudy.
	4	0	54	58	30,04	71		E	1	Cloudy.

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for May, 1812.

1812.	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Hygrometer. °		Winds. Points.	Winds. Str.	Weather.
May 17	9	0	49	57	30,04	73		S	1	Cloudy.
	5	0	52	57	30,01	69		NE	1	Rain.
18	8	0	48	56	29,96	70		ENE	1	Rain.
	4	0	56	58	29,90	70		E	1	Cloudy.
19	8	0	54	56	29,85	76		E	1	Cloudy.
	4	0	62	59	29,74	63		NE	1	Fine.
20	8	0	58	58	29,68	77		W	1	Cloudy.
	4	0	62	59	29,71	61	Rain this Month 1,932 Inches.	W	1	Cloudy.
21	8	0	56	58	29,78	72		W	1	Cloudy.
	4	0	56	59	29,78	69		W	1	Rain.
22	8	0	51	58	29,99	63		W	1	Cloudy.
	4	0	54	58	30,09	59		N	1	Cloudy.
23	8	0	49	57	30,16	59		N	1	Cloudy.
	4	0	57	58	30,26	55		W	1	Cloudy.
24	8	0	52	57	30,32	60		N	1	Cloudy.
	5	0	55	57	30 27	58		S	1	Cloudy.
25	8	0	56	57	30,17	73		W	1	Cloudy.
	8	0	58	58	30,06	70		W	1	Rain.
26	9	0	60	58	29,95	60		W	1	Fine.
	4	0	65	62	29,86	53		SW	1	Cloudy.
27	8	0	63	61	29,62	60		S	1	Cloudy.
	4	0	65	65	29,60	57		S	1	Cloudy.
28	8	0	59	61	29,64	65		SSE	1	Cloudy.
	4	0	64	63	29 66	58		S	1	Cloudy.
29	8	0	61	62	29,64	70		SE	1	Cloudy.
	5	0	66	64	29,63	55		SSE	1	Cloudy.
30	8	0	58	62	29,84	61		W	1	Cloudy.
	4	0	65	64	29,67	55		S	1	Cloudy.
31	8	0	58	62	29.85	58		W	1	Cloudy.
	12	0	58	63	29,82	60		W	2	Rain.

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for June, 1812.

1812	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Hygrometer.		Winds. Points.	Winds. Str.	Weather.
June 1	8	0	58	60	29.73	68°	Rain this Mo' in 1,756 inches.	S	2	Rain. Blowed hard all night.
	4	0	58	61	29,82	67		S	1	Rain.
2	8	0	55	61	30,13	65		SW	1	Fine.
	4	0	64	63	30,00	55		S	1	Cloudy.
3	8	0	59	61	30,05	65		W	1	Cloudy, thick, and hazy.
	4	0	62	62	30,05	62		W by N	1	Cloudy.
4	8	0	59	62	30,09	64		S	1	Fine.
	4	0	66	65	30,10	54		S	1	Fine.
5	8	0	60	62	30,13	60		E	1	Fine.
	4	0	66	65	30,11	55		E	1	Cloudy.
6	8	0	53	62	30 17	65		E	1	Cloudy.
	4	0	63	65	30,14	58		E	1	Fine.
7	8	0	53	61	30,24	62		S	1	Fair.
	4	0	67	64	30,22	53		E	1	Fair.
8	8	0	56	61	30,42	64		NE	1	Cloudy.
	4	0	62	63	30,41	59		N	1	Cloudy.
9	8	0	53	60	30,47	59		N	1	Cloudy.
	4	0	63	64	30,32	55		W	1	Cloudy.
10	8	0	55	61	30,24	60		N	1	Cloudy.
	4	0	56	62	30,29	59		N	1	Cloudy.
11	8	0	56	60	30,29	61		W	1	Cloudy.
	4	0	70	61	30,16	53		W	1	Fine.
12	8	0	60	63	30,09	64		W	1	Cloudy.
	3	0	69	66	30,02	56		W by S	1	Cloudy.
13	8	0	57	63	30,09	64		W	1	Cloudy.
	4	0	66	65	29,93	57		SW	1	Fair.
14	8	0	60	62	29,88	60		W	1	Cloudy.
	5	0	67	65	29,84	54		W	1	Fine.
15	8	0	61	63	29,86	63		SW	1	Cloudy.
	4	0	66	64	29,82	59		S	1	Fine.
16	8	0	57	63	29,80	61		SSW	1	Cloudy.
	4	0	63	64	29,73	54		SSW	2	Cloudy.

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for June, 1812.

1812	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Hygrometer. °		Winds. Points.	Winds. Str.	Weather.
June 17	8	0	52	61	29,56	63		SSE	1	Cloudy.
	4	0	60	62	29,57	60		NW	1	Cloudy.
18	8	0	58	60	29,88	58		W	1	Cloudy.
	4	0	57	61	29,81	67		W	1	Rain. [in the night.
19	8	0	55	60	29,43	69		W	2	Fine. A violent squall of wind
	4	0	62	62	29,39	55		W	1	Fine.
20	8	0	55	60	29,38	53		SW	1	Fine.
	3	0	55	61	29,40	62		W	1	Rain.
21	8	0	55	60	29,58	63		S	1	Cloudy.
	5	0	61	61	29,57	59		SW	1	Cloudy.
22	8	0	52	58	29,74	63		W	1	Cloudy.
	4	0	60	61	29,83	57		W	1	Cloudy.
23	8	0	54	58	29,89	60		W	1	Cloudy.
	4	0	60	60	29,91	55		SW	1	Cloudy.
24	8	0	53	58	30,01	63		W	1	Cloudy.
	4	0	60	60	30,00	56		WNW	1	Cloudy.
25	7	0	55	58	29,99	63		W	1	Cloudy.
	5	0	61	60	29,91	56		W	1	Cloudy.
26	8	0	52	58	29,67	78		E	1	Rain.
	4	0	53	59	29,56	79		N	1	Rain.
27	8	0	51	57	29,92	60		NW	1	Fine.
	4	0	59	60	29,87	53		W	1	Cloudy.
28	8	0	52	58	29,86	63		N	1	Cloudy.
	3	0	57	60	29,96	54		NNE	1	Cloudy.
29	8	0	52	58	30,16	62		W	1	Fine.
	4	0	62	61	30,13	49		N	1	Fine.
30	8	0	57	58	30,06	60		W	1	Cloudy.
	4	0	60	59	29,99	55		W	1	Cloudy.

Rain this Month 1,756 Inches.

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1812	Time.		Therm. without.	Therm. within.	Barom.	Hy-gro-me-ter.		Winds.		Weather.
	H.	M.	°	°	Inches.	°		Points.	Str.	
July 1	8	0	57	59	29,77	69	Rain this Month 1,955 Inches.	W	1	Rain.
	4	0	60	60	29,69	62		W	1	Rain.
2	8	0	58	59	29,48	66		W	1	Cloudy.
	4	0	63	63	29,44	55		W	1	Cloudy.
3	8	0	56	60	29,63	66		N	1	Cloudy.
	4	0	62	62	29,77	62		NE	1	Cloudy.
4	8	0	53	59	30,06	59		E	1	Cloudy.
	4	0	60	60	30,08	51		E	1	Cloudy.
5	8	0	56	59	30,09	59		SW	1	Cloudy.
	4	0	63	61	30,04	61		SW	1	Cloudy.
6	8	0	58	59	30,11	64		W	1	Cloudy.
	4	0	65	62	30,15	53		W	1	Cloudy.
7	7	0	59	61	30,29	64		W	1	Cloudy.
	4	0	67	64	30,30	55		N	1	Cloudy.
8	8	0	62	62	30,35	61		E	1	Fine.
	4	0	67	66	30,36	54		E	1	Fair.
9	8	0	59	62	30,40	64		N	1	Cloudy.
	4	0	67	67	30,35	53		E	1	Fair.
10	8	0	64	64	30,32	61		N	1	Fine.
	4	0	68	69	30,36	69		NNE	1	Cloudy.
11	8	0	60	59	30,44	56		NE	1	Cloudy.
	4	0	67	67	30,34	53		WSW	1	Fair.
12	8	0	64	65	30,15	61		NNE	0	Cloudy.
	3	0	64	59	30,17	54		NNE	1	Cloudy.
13	8	0	55	62	30,18	57		NE	1	Fine.
	4	0	62	64	30,16	51		E	1	Fine.
14	8	0	58	63	30,21	58		E	1	Cloudy.
	4	0	63	65	30,19	54		E	1	Cloudy.
15	8	0	56	62	30,16	61		W	1	Fine.
	4	0	65	65	30,15	50		N	1	Cloudy.
16	8	0	59	63	30,01	59		E	1	Cloudy.
	4	0	65	63	29,90	56		E	1	Cloudy.

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for July, 1812.

1812	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Hygrometer. °		Winds. Points.	Winds. Str.	Weather.
July 17	8	0	63	63	29,95	60	Rain this Month 1,955 Inches.	W	1	Cloudy.
	4	0	65	64	30,06	60		N	1	Cloudy.
18	8	0	63	64	30,16	64		E	1	Fair.
	4	0	69	68	30,13	56		W	1	Fine.
19	8	0	62	65	30,01	65		S	1	Cloudy.
	4	0	64	65	29,84	63		SW	1	Rain.
20	8	0	62	65	29,66	68		W	1	Cloudy.
	4	0	67	67	29,64	67		W	1	Rain.
21	8	0	59	65	29,88	61		W	1	Fair.
	4	0	65	65	29,95	55		S	1	Cloudy.
22	8	0	58	63	29,94	59		NE	2	Cloudy.
	4	0	55	64	29,95	62		N	2	Cloudy.
23	8	0	55	61	30,15	60		W	1	Cloudy.
	4	0	59	64	30,07	61		W	1	Cloudy.
24	8	0	55	62	29,91	62		WSW	2	Cloudy.
	4	0	63	63	29,78	65		S	1	Cloudy.
25	8	0	62	63	29,75	72		W	1	Cloudy.
	4	0	68	66	29,75	60		SW	1	Cloudy.
26	8	0	60	64	29,78	58		W	1	Fair.
	4	0	67	66	29,80	51		W	1	Cloudy.
27	8	0	58	63	29,84	60		W	1	Cloudy.
	4	0	64	63	29,61	66		E	1	Rain.
28	8	0	56	62	29,62	72		S	1	Cloudy.
	4	0	61	64	29,61	56		W	1	Cloudy.
29	8	0	57	61	29,58	62		W	1	Cloudy.
	4	0	63	64	29,55	55		W	1	Cloudy.
30	8	0	55	61	29,82	61		NW	1	Cloudy.
	4	0	63	63	29,89	50		W	1	Cloudy.
31	8	0	56	60	29,96	63		W	1	Fine.
	4	0	63	62	29,92	54		S	1	Fine.

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for August, 1812.

1812	Time.		Therm. without.	Therm. within.	Barom.	Hygrometer.		Winds.		Weather.
	H.	M.	°	°	Inches.	°		Points.	Str.	
Aug. 1	8	0	57	61	29,76	66	Rain this Month 1,430 Inches.	E	1	Cloudy.
	4	0	60	61	29,73	68		N	2	Rain.
2	8	0	57	61	29,86	66		NW	1	Cloudy.
	4	0	64	63	29,88	57		W	1	Cloudy.
3	8	0	58	61	29,77	76		SE	1	Rain.
	4	0	63	62	29,77	62		N	1	Cloudy.
4	8	0	56	61	29,83	69		N	1	Cloudy and thick.
	4	0	62	62	29,86	62		SE	1	Rain.
5	8	0	53	61	29,92	70		W	1	Rain.
	4	0	57	61	29.93	65		N	1	Cloudy.
6	7	0	54	60	29,95	69		N by W	1	Cloudy.
	4	0	60	62	29,93	63		NNW	1	Cloudy.
7	8	0	55	60	29,99	67		W	1	Cloudy.
	4	0	58	62	29,97	61		N	1	Cloudy.
8	8	0	56	60	29,91	67		N	1	Cloudy.
	4	0	57	60	29,89	65		NW	1	Rain.
9	8	0	55	60	29,95	65		NW	1	Cloudy.
	7	0	57	60	29,97	64		N	1	Cloudy.
10	7	0	55	59	29,98	65		N	1	Cloudy.
	5	0	58	60	29,96	63		N	1	Cloudy.
11	8	0	56	59	29,96	73		N	1	Rain.
	4	0	60	60	30,00	67		NNE	1	Cloudy.
12	8	0	54	59	30,13	61		N	1	Cloudy.
	4	0	56	60	30,14	58		N	1	Cloudy.
13	8	0	53	58	30,18	66		N	1	Cloudy.
	9	0	59	61	30,20	63		N	1	Fine.
14	7	0	54	60	30,22	66		N	1	Thick and cloudy.
	4	0	55	63	30,16	63		E	1	Cloudy.
15	7	0	57	61	30,16	65		SE	1	Cloudy.
	4	0	55	63	30,13	65		E	1	Fine.
16	8	0	59	64	30,04	67		E	1	Rain.
	4	0	58	64	30,03	65		E	1	Cloudy.

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1812	Time H.	Time M.	Therm. without. °	Therm. within. °	Barom. Inches.	Hygrometer. °	Rain	Winds. Points.	Winds. Str.	Weather.
Aug. 17	7	0	62	62	30,02	65	Rain this Month 1,480 Inches.	W	1	Fair.
	4	0	68	66	30,02	57		N	1	Fine.
18	8	0	63	64	29,96	65		W	1	Fair.
	4	0	69	71	29,90	54		E	1	Fair.
19	8	0	60	66	29,70	62		WSW	1	Fine.
	3	0	68	69	29,76	59		SW	1	Fine.
20	7	0	60	66	29,98	66		W	1	Fair.
	4	0	68	69	30,13	53		SW	1	Cloudy.
21	7	0	63	66	29,99	61		W	1	Cloudy.
	4	0	67	67	29,92	60		S	1	Cloudy.
22	7	0	61	66	29,96	68		W	1	Fine.
	4	0	67	68	30,02	54		W	1	Fine.
23	8	0	62	66	30,08	65		W	1	Cloudy.
	4	0	68	67	30,02	58		SW	1	Cloudy.
24	7	0	63	66	29,86	66		S	1	Cloudy.
	4	0	66	70	29,98	56		NNW	1	Cloudy.
25	7	0	57	65	30,15	62		W	1	Fine.
	4	0	66	68	30,13	54		S	1	Cloudy.
26	7	0	60	65	30,03	62		W	1	Hazy.
	5	0	67	66	29,97	57		N	1	Cloudy.
27	7	0	57	65	29,94	65		N	1	Fine.
	4	0	65	70	29,93	54		NE	1	Fine.
28	7	0	53	64	30,01	64		NNE	1	Cloudy.
	4	0	58	64	30,04	60		N	1	Cloudy.
29	8	0	53	62	29,99	66		N	1	Rain.
	4	0	56	62	29,99	68		NE	1	Rain.
30	8	0	55	61	30,01	72		N by E	1	Rain.
	4	0	59	65	30,04	69		N	1	Fine.
31	7	0	56	61	30,10	71		N	1	Cloudy.
	4	0	60	62	30,11	60		N	1	Cloudy.

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1812	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Hygrometer.		Winds. Points.	Winds. Str.	Weather.
Sept. 1	7	0	55	61	30,18	63°	Rain this Month 0,634 Inches.	N	1	Cloudy.
	4	0	60	62	30,19	57		N	1	Cloudy.
2	7	0	54	60	30,22	65		NNW	1	Cloudy.
	4	0	62	61	30,19	58		W	1	Cloudy.
3	7	0	57	60	30,12	67		W	1	Hazy.
	4	0	62	61	30,05	66		W	1	Cloudy.
4	7	0	57	60	29,90	65		NE	1	Hazy.
	4	0	62	61	29,86	60		E	1	Cloudy.
5	8	0	56	60	29,87	66		N	1	Fair.
	4	0	62	65	29,89	58		E	1	Fine.
6	7	0	56	61	29,96	71		N	1	Fine.
	3	0	64	65	29,98	55		E	1	Fine.
7	8	0	54	60	30,10	62		N	1	Cloudy.
	4	0	62	72	30,13	55		ESE	1	Fine.
8	7	0	52	61	30,13	65		E	1	Thick and cloudy.
	4	0	64	68	30,04	58		SE	1	Cloudy.
9	7	0	60	62	29,94	66		SE	1	Cloudy.
	4	0	63	64	29,90	65		SW	1	Cloudy.
10	7	0	58	62	30,03	68		W	1	Fine.
	4	0	64	68	30,14	54		NW	1	Fine.
11	8	0	56	62	30,21	54		W	1	Fine.
	4	0	64	68	30,24	55		WNW	1	Cloudy.
12	7	0	52	65	30,30	55		W	1	Cloudy.
	4	0	63	78	30,33	56		NW	1	Fine.
13	8	0	56	63	30,32	64		W	1	Fine.
	3	0	66	71	30,28	55		W	1	Fair, without a cloud.
14	7	0	54	63	30,25	64		W	1	Thick and hazy.
	5	0	63	71	30,21	55		W	1	Fair. No clouds.
15	7	0	51	64	30,16	63		W	1	Fair. No clouds.
	4	0	64	83	30,13	55		WSW	1	Fair.
16	7	0	53	64	30,00	65		W	1	Fair.
	5	0	64	72	29,95	56		W	1	Fair.

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1812.	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Hygrometer.		Winds. Points.	Winds. Str.	Weather.
Sep. 17	7	0	55	64	29,91	65°	Rain this Month 0,634 Inches.	W	1	Cloudy.
	4	0	59	63	29,90	66		N	1	Rain.
18	7	0	48	62	30,07	65		W	1	Fine, rather hazy.
	4	0	56	79	30,20	55		N	1	Fine.
19	7	0	44	60	30,27	63		W	1	Cloudy.
	4	0	57	69	30,23	53		W	1	Cloudy.
20	8	0	54	60	30,19	65		W	1	Fair.
	6	0	59	65	30,17	56		S	1	Fine.
21	7	0	55	61	30,05	62		SSE	1	Fair.
	4	0	62	83	30,01	55		SW	1	Fine.
22	7	0	55	62	30,00	65		W	1	Hazy.
	4	0	63	65	30,00	64		W	1	Cloudy.
23	7	0	54	62	30,04	71		NE	1	Fine.
	4	0	57	62	30,04	60		N	1	Cloudy.
24	7	0	50	60	30,00	66		W	1	Cloudy.
	4	0	55	62	29,99	56		N	1	Fine.
25	8	0	47	58	30,16	62		E	1	Fine.
	4	0	57	61	30,11	57		S	1	Cloudy.
26	7	0	55	60	30,10	74		W	1	Hazy.
	3	0	62	61	30,13	58		W	1	Cloudy.
27	8	0	57	60	30,14	72		W	1	Fair.
	10	0	62	62	30,07	70		W	1	Cloudy.
28	7	0	60	60	29,88	72		W	1	Cloudy.
	5	0	62	62	29,72	75		W	1	Rain.
29	7	0	54	61	29,97	69		N	2	Cloudy.
	4	0	57	61	30,01	65		E	1	Cloudy.
30	8	0	57	61	29,96	76		E	1	Cloudy.
	4	0	59	61	29,88	73		E	1	Cloudy.

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1812	Time. H.	M.	Therm. without. °	Therm. within. °	Barom. Inches.	Hygrometer. °		Winds. Points.	Str.	Weather.
Oct. 1	7	0	58	61	29,80	77	Rain this Month 3,137 Inches.	E	1	Cloudy. [noon.
	4	0	59	62	29,79	72		W	1	Cloudy. A thunder-storm at
2	7	0	47	60	29,99	71		W	1	Fair.
	4	0	59	61	30,00	66		SW	1	Rain.
3	7	0	51	60	30,07	73		SW	1	Hazy.
	4	0	59	65	30,00	63		W	1	Fine.
4	8	0	52	60	29,95	72		S	1	Fair.
	4	0	60	66	29,93	62		S	1	Fair.
5	8	0	54	61	29,86	71		E	1	Foggy.
	4	0	61	62	29,76	67		S	1	Cloudy.
6	7	0	59	62	29,40	71		S	2	Fine.
	4	0	66	63	29,23	62		SE	2	Cloudy.
7	7	0	44	60	29,40	70		W	1	Fair.
	4	0	56	61	29,38	60		SSE	1	Cloudy.
8	7	0	55	60	29,17	75		SE	2	Rain.
	4	0	58	62	29,31	61		SW	1	Cloudy.
9	7	0	49	58	29,44	67		NE	1	Rain.
	4	0	54	59	29,48	65		SW	1	Fine.
10	7	0	50	58	29,40	70		SE	1	Cloudy.
	4	0	55	59	29,41	70		E	1	Cloudy.
11	7	0	48	58	29,36	72		W	1	Rain.
	4	0	54	57	29,31	69		W	1	Cloudy.
12	8	0	45	56	29,27	73		E	1	Foggy.
	4	0	51	56	29,22	71		W	1	Cloudy.
13	7	0	44	55	29,22	71		S by W	1	Cloudy.
	5	0	53	57	29,08	65		E	2	Rain.
14	7	0	49	55	29,88	72		SW	1	Rain.
	4	0	49	55	28,88	69		NNW	1	Cloudy.
15	7	0	45	54	29,19	70		W	1	Fine.
	4	0	52	55	29,22	65		W	1	Fine.
16	7	0	45	54	29,43	71		W	1	Fine.
	5	0	54	55	29,53	61		W	1	Cloudy.

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1812	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Hygrometer. °	Winds. Points.	Winds. Str.	Weather.
Oct. 17	7	0	43	54	29,63	70	E	1	Foggy.
	3	0	52	54	29,36	70	E	1	Cloudy. [in the night.
18	7	0	51	54	29,09	76	SW	1	Cloudy. Much wind and rain
	3	0	54	55	29,13	69	SSW	1	Rain.
19	7	0	51	55	29,77	68	W	2	Rain.
	4	0	55	57	28,63	66	SW	2	Cloudy.
20	7	0	49	55	28,83	65	W	1	Fine.
	4	0	54	56	28,99	58	NW	1	Cloudy.
21	7	0	45	54	29,51	64	NW	1	Cloudy.
	4	0	50	54	29,77	60	NW	1	Hazy.
22	8	0	50	54	29,56	68	S	2	Cloudy.
	4	0	55	58	29,44	66	W	1	Cloudy.
23	7	0	48	55	29,47	66	W	1,2	Cloudy.
	4	0	54	58	29,65	62	W	2	Cloudy.
24	7	0	44	56	29,97	68	W	1	Fine.
	4	0	53	58	29,97	62	W	1	Fine.
25	7	0	47	55	29,82	69	SSE	1	Fair.
	4	0	53	56	29,57	62	SW	1,2	Cloudy.
26	7	0	45	53	29,56	69	SW	1	Cloudy.
	4	0	50	58	29,69	60	W	1	Fair.
27	8	0	43	54	29,79	69	W	1	Cloudy.
	4	0	49	56	29,24	72	S	3	Rain.
28	8	0	42	53	29,44	67	W	1	Fair.
	4	0	47	56	29,47	61	W by N	1	Cloudy.
29	7	0	37	53	29,78	69	W	1	Fine, rather hazy.
	3	0	46	56	29,82	60	W	1	Fair.
30	7	0	44	53	29,79	67	E	1	Cloudy.
	4	0	49	55	29,73	64	SSE	1	Cloudy.
31	7	0	39	54	29,89	72	W	1	Fine.
	3	0	50	58	29,91	61	W	1	Fair.

Rain this Month 3,137 Inches.

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1812	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Hy-gro-me-ter. °		Winds. Points.	Winds. Str.	Weather.
Nov. 1	7	0	50	55	29,98	72	Rain this Month 1,855 Inches.	S	2	Cloudy.
	4	0	52	56	29,96	67		S	2	Cloudy.
2	7	0	51	55	29,90	76		N	1	Rain.
	4	0	52	57	30,01	66		W	1	Cloudy.
3	8	0	47	55	30,09	73		W	1	Rain.
	4	0	49	57	30,12	69		W	1	Fine.
4	7	0	43	53	30,14	69		W	1	Hazy.
	4	0	49	56	30,08	02		W by N	1	Cloudy.
5	7	0	43	55	29,86	67		NW	1	Fine.
	4	0	48	56	29,88	63		NNW	1	Cloudy.
6	7	0	37	54	29,85	68		W	1	Thick and cloudy.
	4	0	44	56	29,84	62		NW	1	Cloudy.
7	7	0	34	51	29,87	68		NW	1	Cloudy.
	4	0	41	54	29,88	65		W	1	Fine.
8	7	0	30	49	29,82	69		E	1	Thick and cloudy.
	11	0	36	50	29,79	66		E	1	Cloudy.
9	8	0	38	49	29,81	69		NE	1	Thick and cloudy.
	4	0	43	51	29.87	64		N	1	Cloudy.
10	7	0	34	49	30,11	69		E	1	Thick and cloudy.
	4	0	42	50	30,16	66		SE	1	Cloudy.
11	7	0	39	49	30,06	68		SE	1	Cloudy.
	4	0	45	52	29,95	64		SW	1	Cloudy.
12	8	0	40	49	29,81	72		E	1	Rain.
	4	0	42	52	29,74	73		E	2	Rain.
13	8	0	46	52	29,59	76		E	1	Thick and cloudy.
	9	0	48	54	29,35	75		S	1	Rain.
14	8	0	48	53	29,22	79		W	1,2	Cloudy.
	4	0	51	56	29,39	70		NE	1	Cloudy.
15	8	0	43	53	29,64	74		W	1	Fine.
	4	0	47	54	29,62	72		SSE	1	Cloudy.
16	8	0	45	53	29,31	75		E	1,2	Cloudy.
	[illegible]	0	45	56	29,20	72		E	2	Rain.

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1812	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Hy-gro-me-ter. °		Winds. Points.	Winds. Str.	Weather.
Nov. 17	8	0	43	53	28,99	75	Rain this Month 1,855 Inches.	E	1	Cloudy.
	4	0	43	56	28,99	76		NE	1,2	Rain.
18	8	0	42	53	29,15	73		NE	1	Cloudy.
	4	0	42	54	29,30	72		NNE	2	Cloudy.
19	8	0	35	51	29,66	68		N	1	Fair.
	4	0	40	53	29,80	70		N	1	Fine.
20	8	0	31	51	29,88	72		N	1	Foggy.
	4	0	39	53	29,87	70		SE	1	Cloudy.
21	8	0	35	51	29,98	66		N	1	Foggy.
	4	0	37	52	30,09	63		N	1	Fine.
22	8	0	33	49	30,33	71		N	1	Fine.
	3	0	38	51	30,37	70		NE	1	Fine.
23	8	0	27	46	30,38	70		S	1	Thick and hazy.
	4	0	47	50	30,30	70		S	1	Cloudy.
24	8	0	40	47	30,12	72		W	1	Fine.
	4	0	43	51	30,00	70		SW	1	Cloudy.
25	8	0	42	48	29,86	74		S	1	Cloudy.
	4	0	43	50	29,79	64		SW	1	Cloudy.
26	8	0	39	48	29,77	69		E	1	Cloudy.
	4	0	45	52	29,76	75		W	1	Cloudy.
27	8	0	46	51	30,07	76		W	1	Cloudy.
	4	0	45	53	30,17	76		W	1	Cloudy.
28	8	0	43	52	30,22	77		E	1	Rain.
	4	0	44	54	30,09	76		E	1	Cloudy.
29	8	0	42	51	29,93	75		E	1	Cloudy.
	4	0	47	52	29,93	75		W	1	Cloudy.
30	8	0	47	51	29,97	78		S	1	Cloudy.
	4	0	50	55	29,97	80		SW	1	Rain.

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1812	Time.		Therm without.	Therm. within.	Barom.	Hy-gro-me-ter.		Winds.		Weather.
	H.	M.	°	°	Inches.			Points.	Str.	
Dec. 1	8	0	49	53	29,91	° 79	Rain this Month 0,188 Inches.	S by E	1	Rain.
	4	0	50	54	29,82	75		S	1	Cloudy.
2	8	0	46	53	29,99	74		W	1	Cloudy.
	4	0	49	57	30,06	70		W	1	Cloudy.
3	8	0	47	54	30,10	76		E	1	Rain.
	4	0	48	57	30,12	76		E	1	Cloudy.
4	8	0	46	55	30,12	75		E	1	Cloudy.
	4	0	48	55	30,09	75		E	1	Cloudy.
5	8	0	41	54	30,11	71		E	1	Fine.
	3	0	42	55	30,12	66		E	1	Cloudy.
6	8	0	38	52	30,29	72		N	1	Fine.
	4	0	41	52	30,34	63		E	1	Cloudy.
7	8	0	32	48	30,54	65		E by S	1	Cloudy.
	4	0	36	52	30,53	65		E	1	Thick and hazy.
8	8	0	28	46	30,42	65		E	1	Cloudy.
	4	0	32	49	30,22	65		E	1	Fine.
9	8	0	25	45	29,95	68		W	1	Cloudy and hazy.
	4	0	35	49	29,95	63		NE	1	Cloudy.
10	8	0	31	44	29,91	75		W	1	Cloudy.
	4	0	35	48	29,81	72		W	1	Thick and hazy.
11	8	0	33	46	29,88	71		E	1	Cloudy. Snow in the night.
	4	0	34	47	29,98	70		E	1	Cloudy.
12	8	0	31	45	29,97	70		E	1	Cloudy. Snow in the night.
	4	0	32	47	29,89	67		E	1	Cloudy.
13	8	0	25	43	29,78	70		NE	1	Fine.
	4	0	31	44	29,73	69		NE	1	Fair.
14	8	0	27	42	29,72	72		NE	1	Cloudy.
	4	0	34	44	29,68	74		N	1	Cloudy.
15	8	0	30	40	29,66	68		E	2	Cloudy.
	4	0	32	43	29,52	68		ESE	2	Cloudy.
16	8	0	28	40	29,21	68		E	1	Cloudy.
	4	0	29	42	29,03	70		N	2	Cloudy.

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1812	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Hygrometer. °		Winds. Points.	Winds. Str.	Weather.
Dec. 17	8	0	33	41	28,95	75	Rain this Month 0,188 inches.	E	1	Cloudy. Much snow in the night.
	4	0	35	43	29,04	77		E	1	Cloudy.
18	9	0	33	42	29,25	72		E	1	Rain.
	4	0	35	44	29,29	76		W	1	Rain.
19	9	0	37	43	29,52	78		E	1	Cloudy and foggy.
	4	0	38	44	29,52	78		E	1	Foggy and cloudy.
20	9	0	36	44	29,58	78		E	1	Cloudy.
	4	0	36	43	29,56	74		E	1	Cloudy.
21	8	0	32	42	29,72	74		N	1	Cloudy.
	4	0	35	44	29,77	72		W	1	Cloudy.
22	8	0	37	43	29,82	75		E	1	Cloudy.
	4	0	38	45	29,87	78		E	1	Cloudy.
23	8	0	35	43	30,02	77		N	1	Cloudy.
	4	0	37	46	30,09	78		E	1	Rain.
24	8	0	33	43	30,30	73		N by E	1	Cloudy.
	4	0	36	46	30,33	72		N	1	Cloudy.
25	9	0	32	47	30,47	67		NE	1	Cloudy.
	4	0	33	47	30,45	67		N	1	Cloudy.
26	9	0	33	43	30,41	74		N	1	Hazy.
	4	0	37	47	30,37	73		NE	1	Cloudy.
27	9	0	33	44	30,50	75		NE	1	Cloudy.
	4	0	35	44	30,49	73		N	1	Cloudy.
28	9	0	32	42	30,49	72		W	1	Cloudy.
	4	0	33	44	30,46	73		W	1	Fine.
29	9	0	41	43	30,32	76		W	1	Cloudy.
	4	0	45	47	30,21	73		W	1	Cloudy.
30	9	0	43	46	30,18	74		W	1	Fine.
	4	0	45	48	30,15	71		W	1	Cloudy.
31	9	0	42	47	29,95	71		WNW	1	Cloudy.
	4	0	44	50	29,82	72		W	1	Cloudy.

1812.	Thermometer without.			Thermometer within.			Barometer.*			Hygrometer.			Rain.†
	Greatest height.	Least height.	Mean height	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	
	Deg.	Deg.	Deg.	Deg.	Deg.	Deg.	Inches.	Inches.	Inches.	Deg.	Deg.	Deg.	Inches.
January	47	30	38,3	53	43	47,1	30,66	29,18	29,88	75	61	70,2	0,779
February	53	35	43,8	57	50	52,6	30,12	29,20	29,66	77	60	69,8	1,702
March	55	32	41,7	57	44	50,3	30,38	28,94	29,77	76	59	66,8	1,870
April	53	37	44,9	58	50	53,4	30,22	29,49	29,92	76	54	63,6	1,060
May	66	47	55,1	65	55	58,9	30,32	29,57	29,87	77	52	64,0	1,932
June	70	51	58,6	66	57	61,1	30,47	29 38	29,94	79	53	60,2	1,756
July	69	53	61,3	69	59	63,0	30,44	29,44	29,96	72	50	59,8	1,955
August	69	53	59,4	71	58	63,0	30,22	29,70	29,98	76	53	63,6	1,480
September	64	44	57,7	83	58	64,1	30,33	29,72	30,07	76	53	62,5	0,634
October	59	37	50,7	65	53	57,1	29,53	30,07	28,63	77	58	67,3	3,137
November	52	27	42,5	55	46	52,3	30,38	28,99	29,85	80	62	70,7	1,855
December	50	25	36,5	57	40	46,8	30,54	28,95	29,96	79	63	72,0	0,188
Whole year			49,2			55,8			29,79			65,8	18,348

* The quicksilver in the bason of the barometer, is 81 feet above the level of low water spring tides at Somerset-house.

† The Society's Rain Gage is 114 feet above the same level, and 75 feet 6 inches above the surrounding ground.

By another Rain Gage placed 11 feet 6 inches lower, the quantity of rain appears to have been 22,03 inches.

Variation of the Magnetic Needle, October, 1812, 24° 16′ 30″ West.

PHILOSOPHICAL

TRANSACTIONS,

OF THE

ROYAL SOCIETY

OF

LONDON.

FOR THE YEAR MDCCCXIII.

PART II.

LONDON,

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MDCCCXIII.

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Read March 4, 1813.

Sir,

My late brother, in digging clay near Brentford, for the manufacture of bricks and tiles, frequently found the remains of animals and extraneous fossils, which you, Sir, with other Gentlemen of the Royal Society, very often inspected, and the interest which you took in them added much to his ardour in collecting them.

A few days before he was attacked with the illness which terminated in his death, he had drawn up an account of the different strata in which the organic remains in his cabinet were found deposited, with the intention of submitting it to you. His long illness and other circumstances have delayed my communicating to you earlier the enclosed letter, which I found amongst his papers; and I request, Sir, that if you

should deem it of sufficient interest, you will do me the honour of laying it before the Royal Society.

I have the honour to be, Sir,
with great respect,
your much obliged humble servant,
JAMES R. TRIMMER.

Brentford, Feb. 1813.

To the Right Hon. Sir Joseph Banks, *Bart. &c. &c. &c.*

SIR,

My collection of organic remains having been nearly formed under your own eye, and my zeal in the collection, and care in the preservation of them, having been greatly increased by the importance which yourself and other Gentlemen of the Royal Society have attached to them, I do myself the honour of presenting to you the following statement of the manner in which they were deposited in the strata where they have been found; conceiving that however curious the specimens may be in themselves, they can conduce but little to the advancement of geological knowledge, if that part of their history is wanting.

The specimens have been collected from two fields, not contiguous to each other; therefore to avoid confusion, I shall take each field separately, first describing the strata as far as they have come within my knowledge, and afterwards I shall speak of the organic remains as they were respectively found in those strata.

The first field is about half a mile north of the Thames at Kew bridge; its surface is about twenty-five feet above the

Thames at low water. The strata here are first, sandy loam from six to seven feet, the lowest two feet slightly calcareous. Second, sandy gravel a few inches only in thickness. Third, loam slightly calcareous from one to five feet; between this and the next stratum, peat frequently intervenes in small patches of only a few yards wide, and a few inches thick. Fourth, gravel containing water; this stratum varies from two to ten feet in thickness, and is always the deepest in the places covered by peat; in these places the lower part of the stratum becomes an heterogeneous mass of clay, sand, and gravel, and frequently exhales a disagreeable muddy smell. Fifth, the main stratum of blue clay, which lies under this, extends under London and its vicinity, the average depth of this clay has been ascertained, by wells that have been dug through it, to be about two hundred feet under the surface of the more level lands, and proportionably deeper under the hills, as appears from Lord SPENCER's well at Wimbledon, which is five hundred and sixty-seven feet deep. This stratum, besides figured fossils, contains pyrites and many detached nodules; at the depth of twenty feet there is a regular stratum of these nodules, some of which are of very considerable size.

In the first stratum, as far as my observation has extended, no remains of an organised body has ever been found, and as my search has not been very limited, I may venture to say it contains none. In the second stratum, snail shells, and the shells of river fish have been found, and a few bones of land animals, but of inconsiderable size, and in such a mutilated state, that it cannot be ascertained to what class they belong. In the third stratum, the horns and bones of the ox, and the horns, bones, and teeth of the deer have been found, and also, as

in the second stratum, snail shells and the shells of river fish. In the fourth stratum were found teeth and bones of both the African and Asiatic elephant, teeth of the hippopotamus, bones, horns, and teeth of the ox.

A tusk of an elephant measured, as it lay on the ground, nine feet three inches, but in attempting to remove it, it broke into small pieces. When this stratum dips into the clay, and becomes a mixed mass, as before stated, it is seldom without the remains of animals. In the fifth stratum, namely the blue clay, the extraneous fossils are entirely marine, with the exception of some specimens of fruit and pieces of petrified wood, the latter of which may be considered as marine, because when of sufficient size, they are always penetrated by teredines. The other fossils from this stratum are nautili, oysters, pinnæ marinæ, crabs, teeth and bones of fish, and a great variety of small marine shells; this stratum has been penetrated hitherto in this field only to the depth of thirty feet, throughout which the specimens found were dispersed, without any regularity.

The second field is about one mile to the westward of the former, one mile north of the Thames, and a quarter of a mile to the eastward of the river Brent; its height above the Thames, at low water, is about forty feet. The strata are, first, sandy loam, eight or nine feet, in the lowest three feet of which it is slightly calcareous. Second, sand, becoming coarser towards the lowest part, and ending in sandy gravel from three to eight feet. Third, sandy loam highly calcareous, having its upper surface nearly level, but gradually increasing in thickness from a feather edge to nine feet. Below this are two strata of gravel and clay, as in the other field, but as these strata have been only occasionally pene-

trated in digging for water, nothing therefore is known with respect to them, but that they exist there.

In the first stratum, as in the other field, no organic remains have been observed. In the second, but always within two feet of the third stratum, have been found the teeth and bones of the hippopotamus, the teeth and bones of the elephant, the horns bones, and teeth of several species of deer, the horns, bones, and teeth of the ox, and the shells of river fish.

The remains of hippopotami are so extremely abundant, that in turning over an area of one hundred and twenty yards in the present season, parts of six tusks have been found of this animal, besides a tooth and part of the horn of a deer, part of a tusk, and part of a grinder of an elephant, and the horns with a small part of the skull of an ox. One of these horns I had an opportunity of measuring, as it lay on the ground, and found it to be four feet and a half in length, following the curve, and five inches in diameter at the large end; it was found impracticable to remove it, otherwise than in fragments, which I have preserved, and have hopes of being able to put a considerable part of it together. The immense size of this horn is rendered more remarkable, by another horn from the same spot, which measures but six inches in length. Though this stratum is so extremely productive of the remains of animals, yet there are but few good cabinet specimens from it, owing, it is presumed, to their having been crushed at the time they were buried, and to the injury they have since received from moisture. It is necessary to remark, that the gravel stones in this stratum do not appear to have been rounded in the usual way by attrition, and that the bones must have been deposited after the flesh was off,

because, in no instance have two bones been found together which were joined in the living animal; and further, that the bones are not in the least worn, as must have been the case had they been exposed to the wash of a sea beach.

In the third stratum, viz. calcareous loam, have been found the horns, bones, and teeth of the deer, the bones and teeth of the ox, together with snail shells and the shells of river fish.

Brentford, in the neighbourhood of which are the fields I have mentioned, is situated on the north bank of the Thames, and is six miles west of London.

The fall of the Thames from Brentford to its mouth at the Nore,* is estimated at seven feet.

I have the honour to be,
with great respect, Sir,
your much obliged
and obedient humble servant,

WM. KIRBY TRIMMER.

Brentford.

* Col. MUDGE, Trigonometrical Survey, p. 85.

Fig. 1.

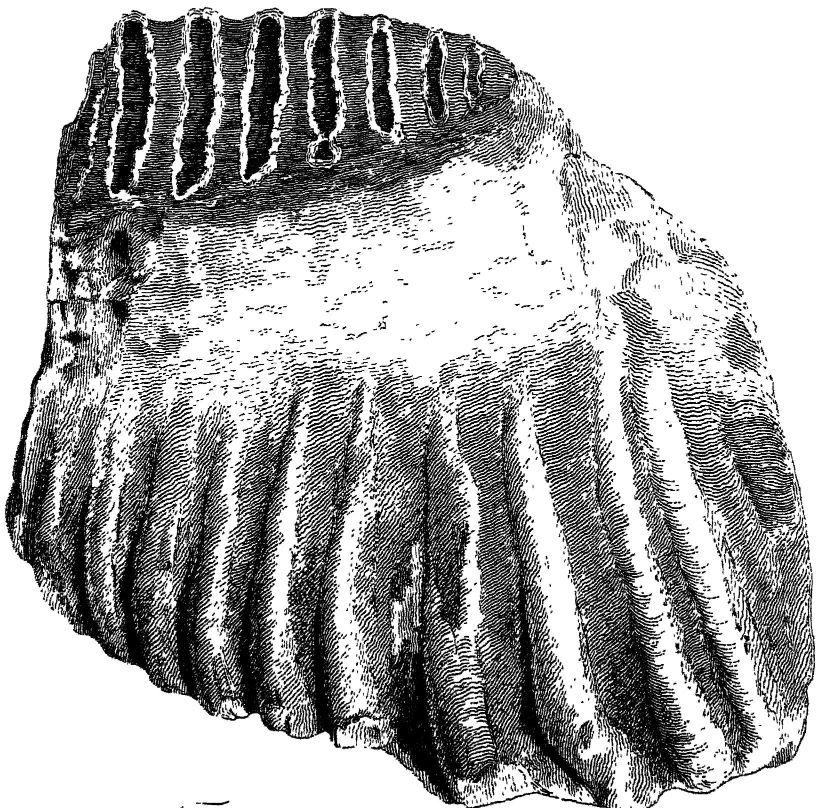
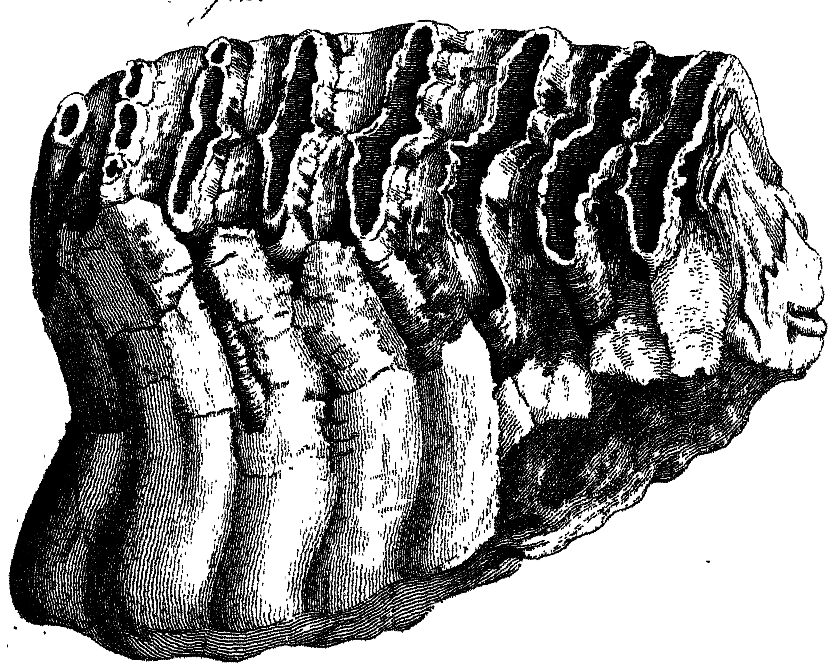


Fig. 2.



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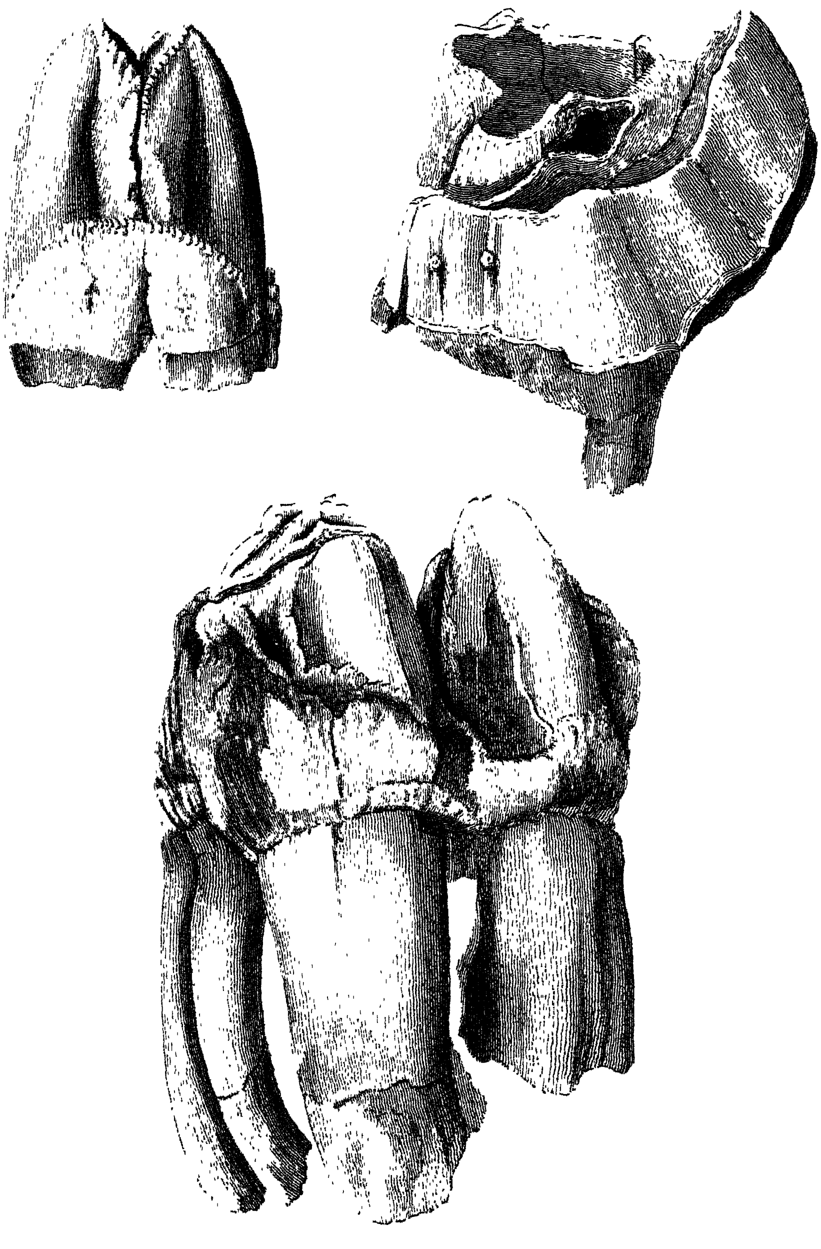

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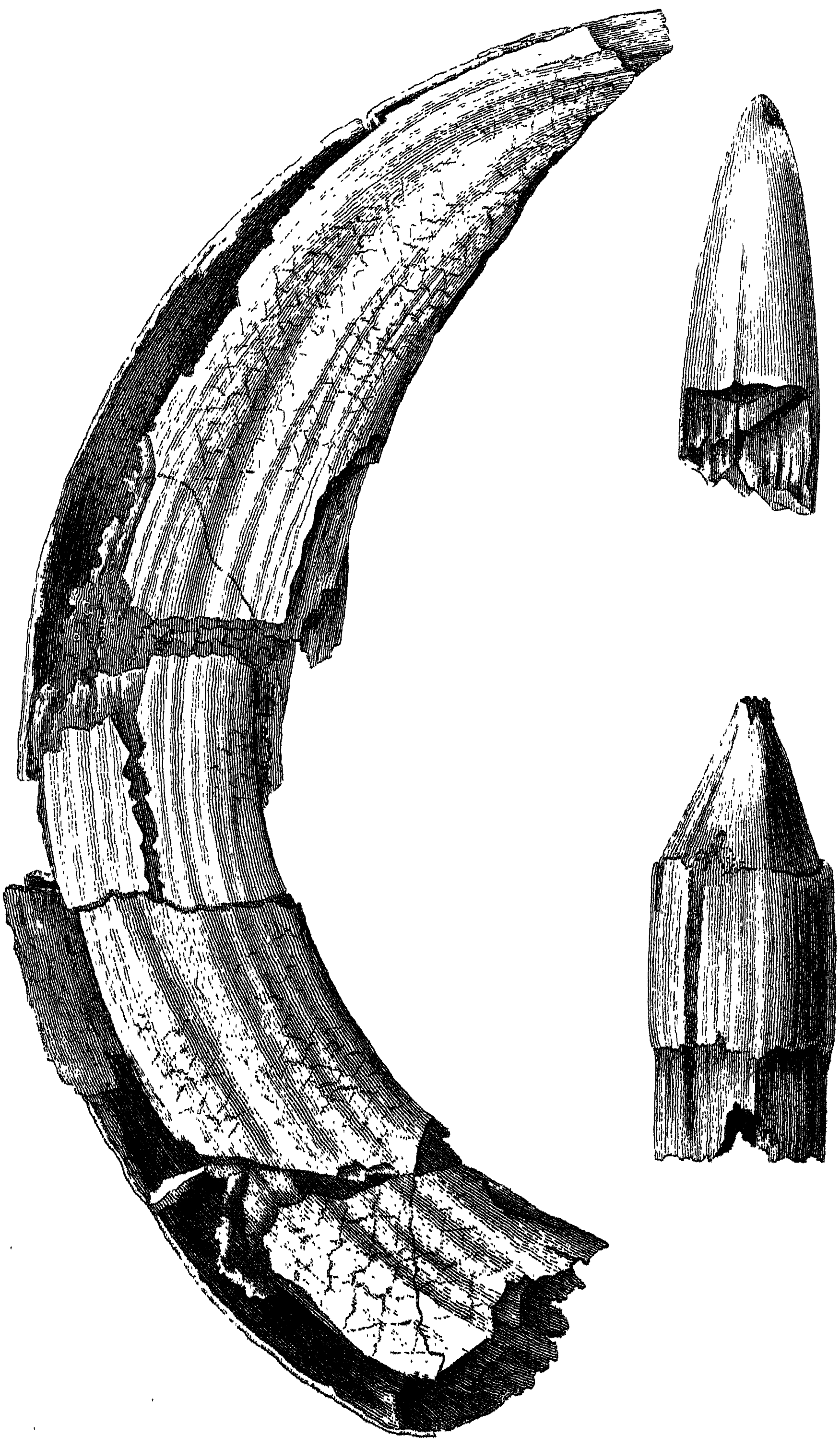

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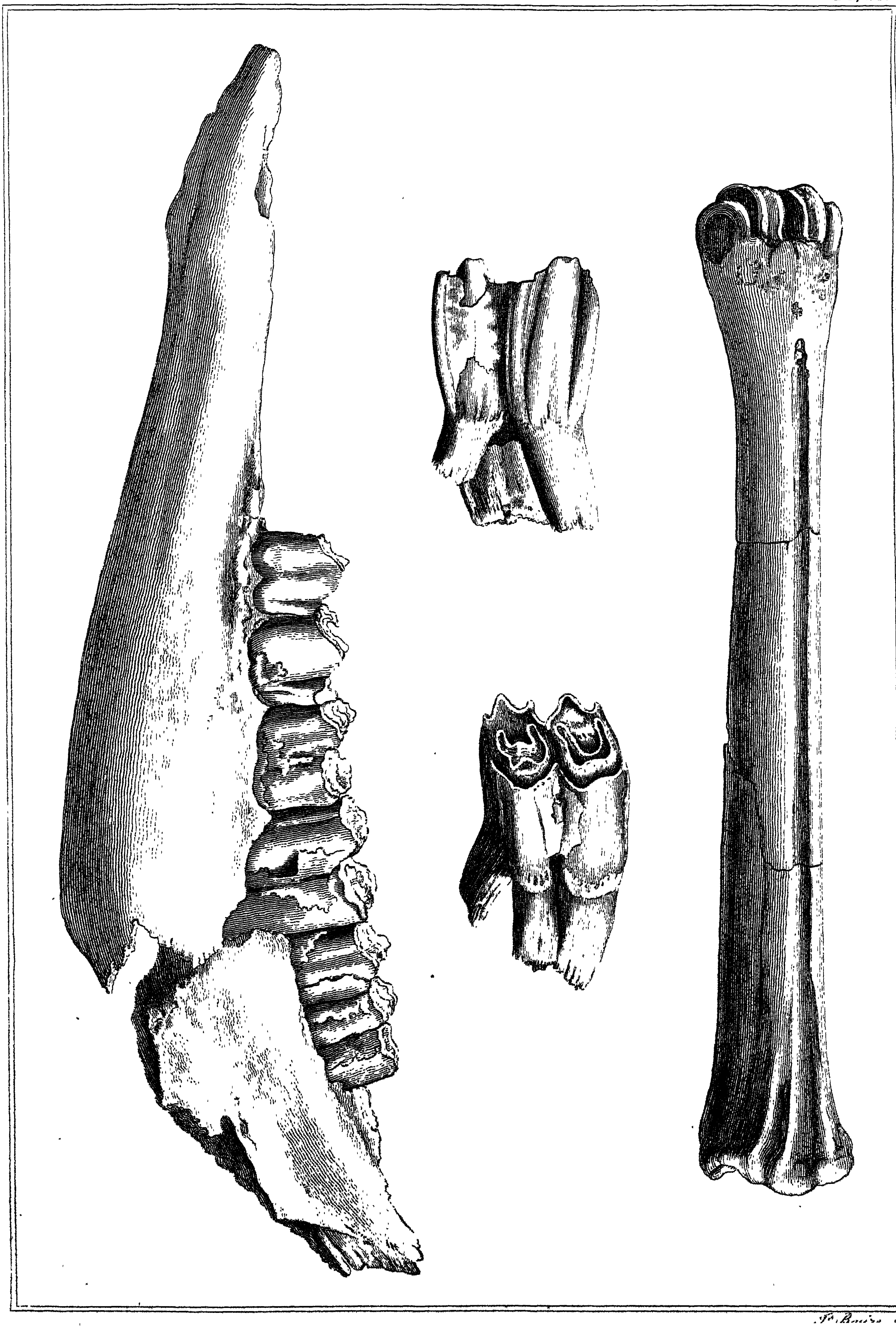
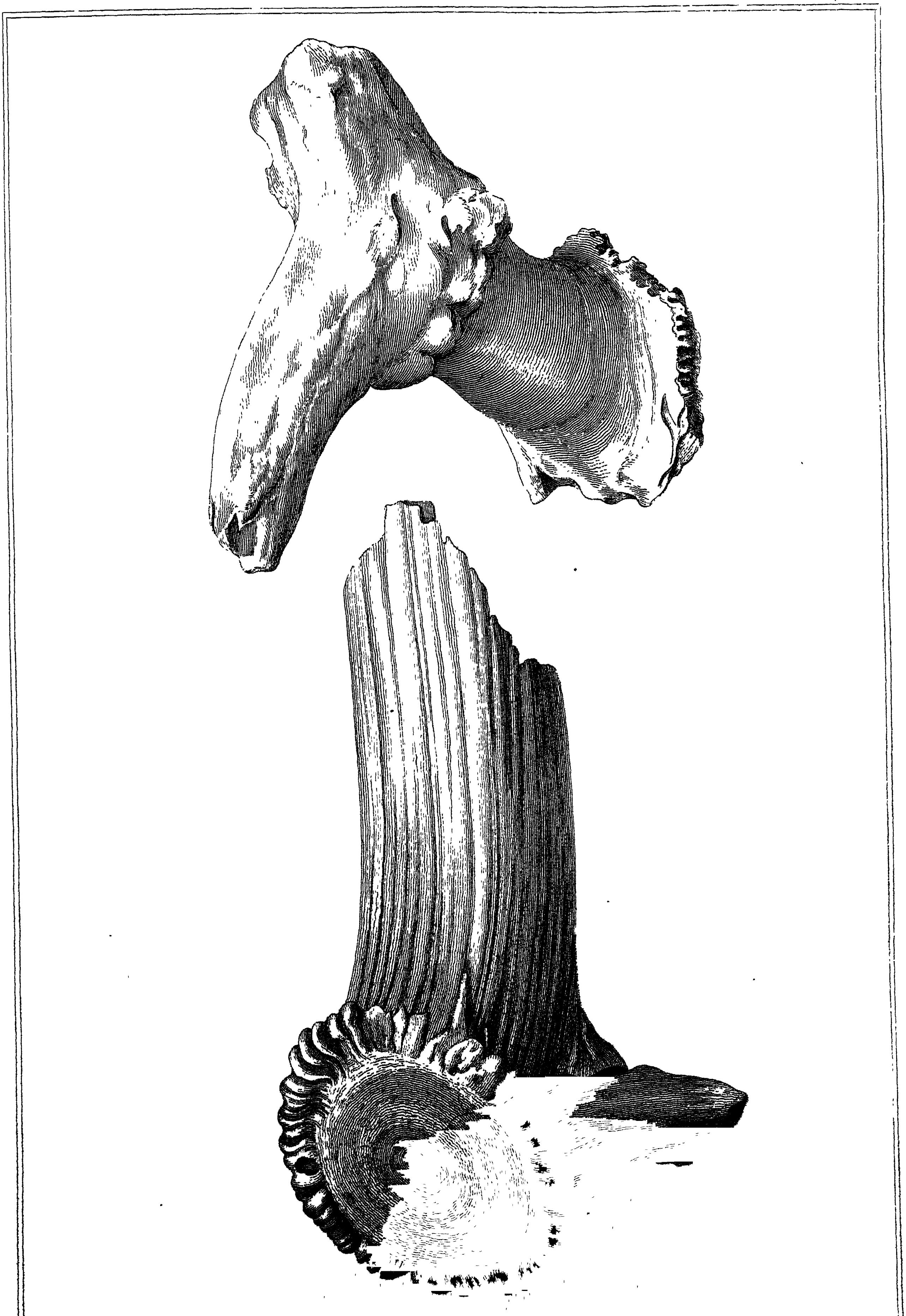




F. Bazaire sc.

EXPLANATION OF THE PLATES.

The following sketches (see Plates VIII, IX, X, XI, XII.) will give a tolerably correct idea of the state in which the bones were found.

Plate VIII.

Fig. 1. The grinding tooth of an Asiatic elephant.
Fig. 2. The grinding tooth of an African elephant.

Plate IX.

Three grinding teeth of the hippopotamus.

Plate X.

A tusk and fore tooth of the hippopotamus.

Plate XI.

Bones of the deer kind. A lower jaw, two grinding teeth, and a bone of the leg.

Plate XII.

Two portions of the horns of the deer. One set upon a bony pedicle, which is not the case in any recent deer, except in a small species found in Prince's Island in the East Indies.

XX. *On a new Construction of a Condenser and Air-pump. By the Rev.* Gilbert Austin. *In a Letter to Sir* Humphry Davy, *LL.D. F.R.S.*

Read March 11, 1813.

Dear Sir,

Woodville, Dec. 5, 1811.

According to your desire, I give you a description of my glass condenser, which, upon inspection, you were so obliging as to consider might possibly be found useful in some of those extensive and profound chemical researches, in which you are happily engaged.

In the year 1799, I sent to the Royal Irish Academy the description of a little condensing instrument for the extemporaneous preparation of artificial mineral waters, which I stated would be more proper for nice experiments, if it could be made entirely of glass. As this suggestion had not, to my knowledge, induced any one better qualified to construct such an instrument of that material; pursuing my own ideas, I contrived the glass condenser herein described, the different parts of which, as to their fittings and application, that is the grinding of the barrel, the fitting and connecting of the flat joints and valves, I have been obliged to execute myself.

I have found this instrument to answer my immediate purpose very well, and have also applied it successfully to some other trivial chemical experiments; but it has for a considerable time (on account of my other avocations) remained neglected, and would have continued so to remain, had not you

given me hopes that it might be admitted to a place in your laboratory.

It was my wish, in the construction of this instrument, to have made the piston of glass only, without any packing of flax or thread for fitting it to the barrel; but I have not ventured to attempt so much. And perhaps as the packing is only a very small ring of a known substance, and as that may be varied at pleasure, by using for the purpose silk, or wool, or leather, or elastic gum, the effect in any experiment may be easily appreciated. The water which lubricates the barrel, and with which the joints are wetted in the first instance only (for the permanent junctures require no second wetting, but remain perfect for any length of time) may require also to be taken into the account, and if on occasion it should be thought preferable, oil may be used in the place of water. Should it however appear to you, that the want of a piston of glass, which should act without the addition of any foreign substance, is an important defect, you may possibly get one executed perfectly by some of those ingenious and excellent artists who are to be found in London. You will probably also improve on the form and construction of this first instrument (as far as I know) made of glass for the purposes mentioned. I have myself partly executed another of more general application on similar principles, of which, with your kind permission, I shall give you an account, as soon as I have completed it.

The great impediment to the forming of pneumatic instruments of glass, appears to me to have arisen from the difficulty of making the necessary joinings, in such a manner as to be capable of sustaining very considerable pressure, to be easily

disunited or connected, as occasion might require, and to be perfectly air tight. The conical junctures hitherto used, do not bear much pressure, and are objectionable in other respects: I have therefore formed my joints of plane surfaces, and find them as perfect, as permanent, and as easy in application as I can desire. For the construction of these joints, it is necessary that the glass vessels, or pieces to be connected, should be so formed that the part where they are to be joined should admit of having a broad projecting ring cast upon it. The ring should be formed by the glass-blower, as truly square and flat as convenient. The face is then to be ground (but not polished) perfectly plane, and then it will of course fit any other plane surface;—a great convenience on many occasions, and an advantage peculiar to the flat joint.

The connecting pieces, or collars for the joints, are formed of a flat ring of brass for each, about half an inch broad, and above one-eighth of an inch thick; this ring must be just wide enough to pass over the glass ring to which it is adapted. Fig. 3, *a* (Pl. XIV.) A mahogany ring about half an inch thick, having the diameter of the hole equal to that of the neck of the vessel above the glass ring, is to be screwed to the brass, and then sawed into two pieces through its diameter, fig. 3, *b*. These pieces are to be unscrewed, and applied to the neck, and they are then to be screwed to their places on the brass. The compound ring, or collar, will be thus secured above the glass ring, and may be fastened to another collar, attached in the same manner to the piece to be joined to it, by four screws passing through both when the joint is intended to be in some degree permanent, as in fig. 3, *c*. in which (*cc*) represents the glass pieces to be joined, (*aa*) the brass ring, and (*bb*) the

ring of mahogany. But if the joint require to be frequently separated in using the instrument, it may be applied moistened to the other, and pressed closely and perfectly by three milled-headed screws in the brass bridge, which bear upon the collar or ring of brass with their points. The degree of pressure necessary is not to be very great, but it must be equably exerted and felt on each screw, in order that the surfaces should be applied truly, and that the joint may be air tight—a very little practice will render this operation easy and certain. The bridge with its screws, &c. is represented, fig. 4, *a* (Pl. XIV.)—The plan of the bridge, and the end of the barrel with the valve in the piston, is seen at (*b*).—(*c*) is a stop to prevent the piece to be joined to the barrel from being pushed too far.

The glass valves are formed of plane convex lenses not polished, fitted to hollow spherical cavities, as in figs. 5 and 6.

These particulars being premised, the description of the instrument will be more easily understood.

Fig. 1, (Pl. XIII.) Represents a perspective view of the glass condenser, mounted on its frame and stand.

Fig. 2, (Pl. XIII.) A side view of the glass pieces of the condenser, disencumbered of the frame and connecting collars. The piston is seen outside of the barrel, and the iron sliding bar, with its arms which support the barrel, is represented with its clamps—but without the mahogany pieces. The same letters refer to the same parts of the instrument in figs. 1 and 2.

In figure 2 are also represented the valves in their places—the joints connected by dotted lines are permanent.

Figs. 1 and 2.

(*a*) The upper vessel which contains the liquid to be impregnated with the gas. A safety valve closes its mouth, seen at large, fig. 5 (Pl. XIV.)

(*b*) A piece connected permanently to the upper vessel; it carries the valve, which is so far below its surface as to allow it to rise a little in action, but not to turn over: the plane side of the lens is indented with a deep cross cut, to allow the gas to pass over it into the upper vessel, seen at large, fig. 6, *g. d* (Pl. XIV.)

(*c*) The glass barrel having a broad ring on each end.

Length of the barrel	-	13. inches.
Diameter of the bore	- -	0.$\frac{7}{8}$
External diameter	-	1.$\frac{3}{4}$
Diameter of the broad rings	-	2.$\frac{1}{4}$

(*d*) The hollow glass piston having one broad ring next (*e*), for the flat joint which connects the piston with it, and another ring, about four inches above this last, in order to secure it the better to the frame in which this ring and the piston are more than half sunk, and then covered over by a piece of mahogany pressing upon it with four screws. The covered part of the piston is represented by the dotted lines near (*d*) in fig. 1. At the end of the piston are two small rings, to confine the packing by which it is fitted to the barrel; and the extremity is ground into the hollow segment of a sphere, into which is fitted an unpolished plane convex lens, fig. 2, and seen at large, fig. 6.

(*e*) The receiver, from which the gas is drawn up and condensed into the upper vessel (*a*). It is furnished with a ground stopper, which may be used occasionally as hereafter mentioned.

(*f*) The pneumatic trough placed on a stool under the receiver.

(*g*) A retort, in which the gas is generated as usual.

(*hh*) Two pieces of mahogany, touching the sides of the barrel, and in their length fitting between its rings, so as when raised or depressed, to move the barrel without shake, and supporting the platform (*i*) with its bridge, as seen at large, fig. 4. These pieces are held together by iron clamps hollowed, so as to admit the barrel freely, fig. 2, *k*: they are clamped behind in the same manner, and then the barrel is secured by wedges in its proper situation, so as to be fixed perfectly parallel to the iron sliding bar. From the clamps behind proceed two strong arms (*kk*), fixed at right angles in the sliding bar (*l*), which moves equably in the long brass dove-tailed groove (*m*). This dove-tailed groove is screwed to the strong upright pillar (*o*). By means of the handle (*n*), which is fastened to the sliding bar between the two strong arms (*kk*), the barrel, with its apparatus, the platform (*i*), the brass bridge confining the upper vessel (*a*), by the piece (*b*), are alternately raised and depressed. In this action, the barrel moves upon the fixed hollow piston (*d*). The gas is extracted from the receiver at every ascending stroke, and passing through the hollow glass piston (*d*) above its valve, which opens upwards, is forced at every descending stroke through the valve between (*a* and *b*) into the upper vessel containing the liquor to be impregnated. Should more gas be forced into the vessel than the liquid readily absorbs, the safety valve at the top (seen at large, fig. 5) allows it to escape.

Fig. 5, (Pl. XIV.) is an enlarged view of the mouth of the upper vessel (*a*), with the collar (*b*) on its neck, hollowed

into a spherical cavity at (*c*) to fit the spherical valve of glass (*d*). This valve is set loosely in the brass piece (*e*), that it may, when pressed into the cavity, accommodate itself to its proper place. The brass piece (*e*) has a long tail (*f*), which passes freely through the milled-headed screw (*g*), and is surmounted at (*f*) with a small button, with which the valve may be raised, and which prevents it from falling out, when the whole frame is detached from the collar (*b*), in order to empty or fill the upper vessel (*a*, fig. 1). On the stem (*f*) of the valve (*d*), below the milled screw, a coiled spring wire is fixed, the pressure of which on the valve may be regulated by the milled screw. The flanch of the frame (*h*) has notches cut into it, which fall into corresponding notches in pieces fixed to the ring of the collar. The notches and the whole flanch pass under these pieces with a circular motion, and are thus secured till turned round in such a manner, as that the notches shall again correspond.

When the collar is not secured by the valve, it drops loosely down on the neck of the upper vessel (*a*), as represented in the figure at (*x*).

Fig. 6, (Pl. XIV.) an enlarged view of the hollow glass piston. (*a*) part of the piston rod. (*bb*) two small rings to hold the packing. (*c*) the packing. (*d*) the valve, an unpolished plane convex lens, fitted to the end of the piston. (*f*) the plan of the end of the piston, the latter is placed on the plane side of the lens. (*eeee*) notches in the upper small ring of the piston, over which are passed crossed threads, in order to confine the valve (*f*), and to prevent it in action from being carried off the end of the piston. The notches are necessary to guard the threads from the attrition of the barrel.

Fig. 1.

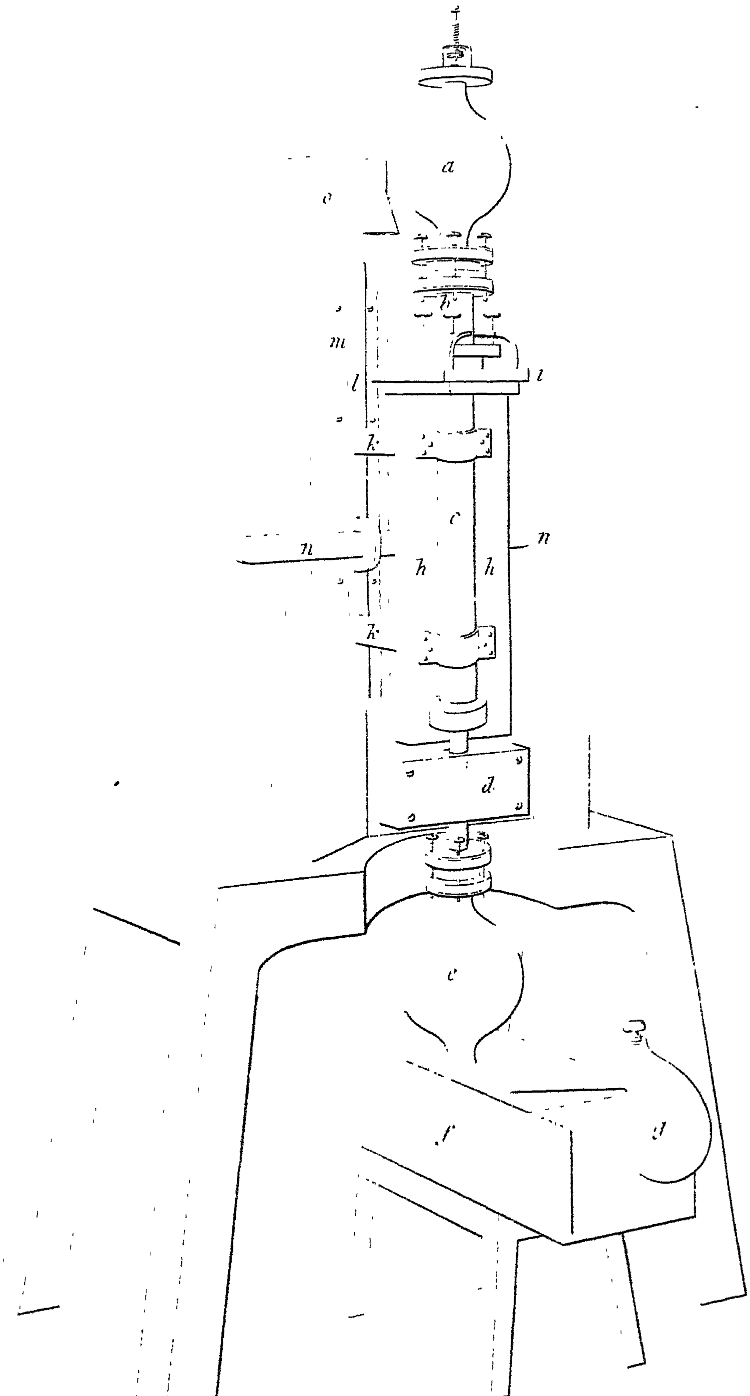
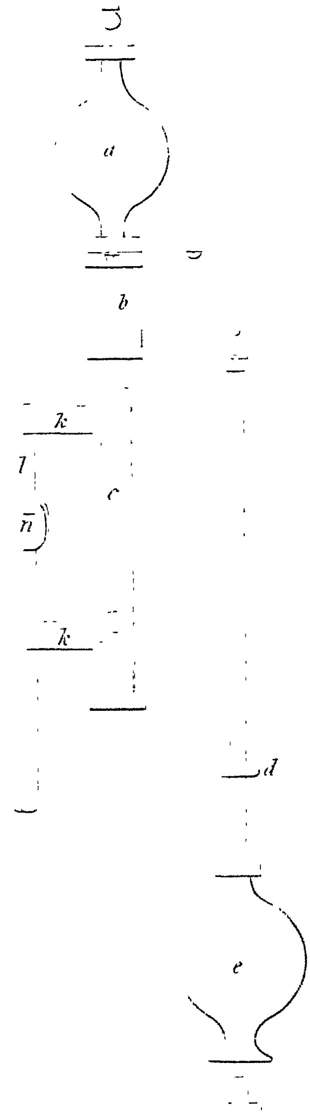


Fig. 2.



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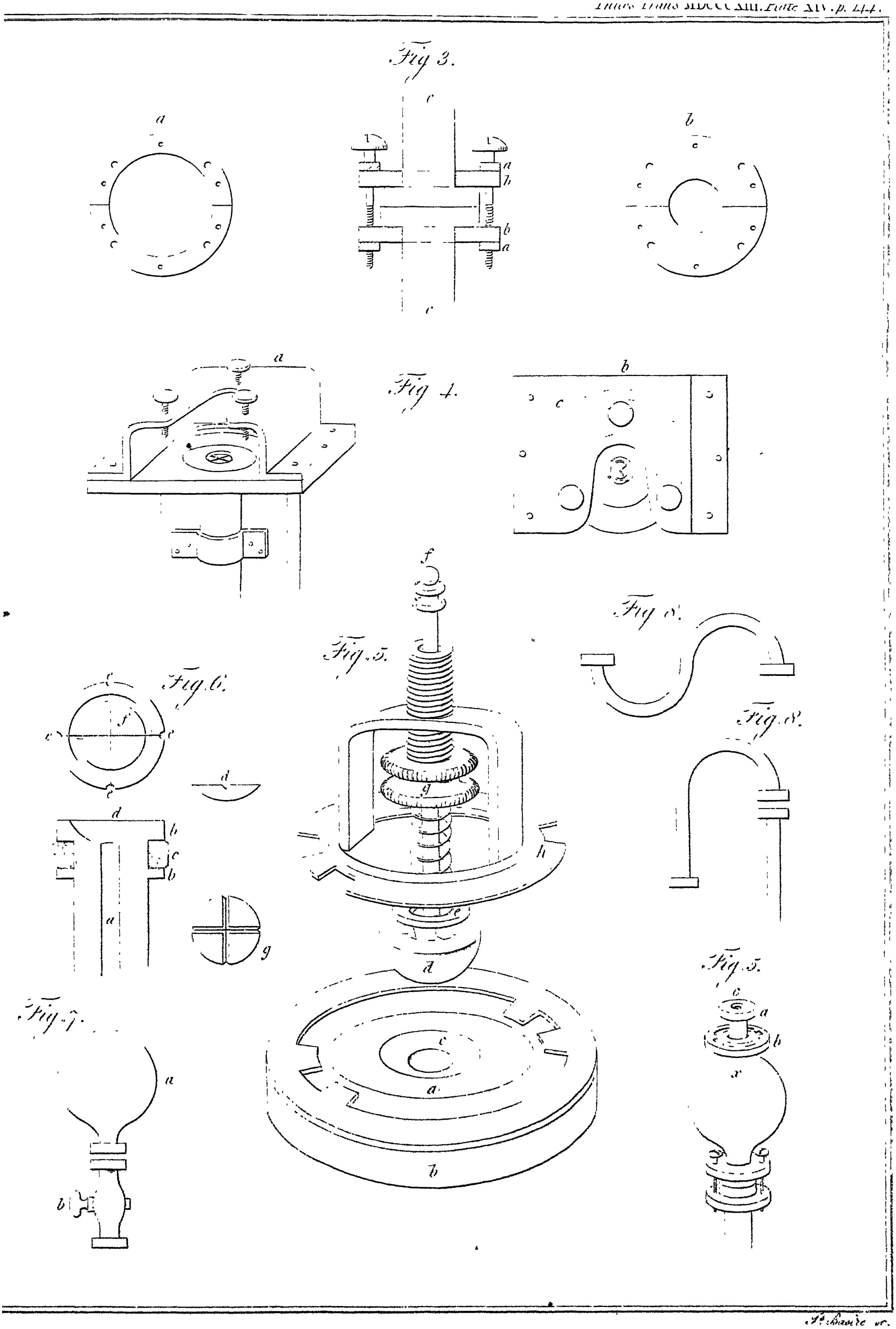


Fig 3.
c
a
c
b
a
b
a
b
c
Fig 4.
a
b
c
f
Fig. 5.
Fig. 6.
c
f
c
c
d
d
b
c
b
a
g
g
h
e
d
Fig 8.
Fig. 8.
Fig. 7.
a
c
a
b
Fig. 5.
c
a
b
x
a
b

J. Basire sc.

Fig. 7, (Pl. XIV.) (*a*) a glass vessel with only one aperture to be used instead of the upper vessel (*a*) when the safety valve is considered unnecessary. For the same purpose the receiver (*e*) figs. 1 and 2, may also be used occasionally, as all the joints fit each other. (*b*) a glass stop-cock to be joined occasionally by a pair of collars to the vessel (*a*) in this figure, or to any other at pleasure. This vessel (*a*), with its stop-cock being joined to the bottom of the piston (*d*, fig. 1.), and in the place of the receiver (*e*) may be exhausted if required. But in order to apply the instrument to the purposes of exhaustion, the bottom of the piston should be set in a platform, and have under it a bridge reversed, similar to that represented in fig. 4.—If the instrument were so constructed, bent tubes with rings of glass and collars might be applied by their flat joints, and would answer for exhausting jars connected with them.

Fig. 8, (Pl. XIV.) tubes differently bended, which may be connected at pleasure by flat joints, so as to form syphons of any size, and conducting pipes in any direction, and of any length.

I have to apologize for the minute particularity of this communication; but as I wish artists to understand me perfectly, I have preferred to be rather prolix, than obscure.

I am, dear Sir, with high esteem and respect,

your humble servant,

GILBERT AUSTIN.

XXI. *On the Formation of Fat in the Intestines of living Animals. By Sir* Everard Home, *Bart. Presented by the Society for promoting the Knowledge of Animal Chemistry.*

Read March 18, 1813.

THE investigation of the digestive organs of different animals, in which I have been engaged for many years, has led me imperceptibly into an enquiry respecting the particular uses of the lower portion of the intestines in birds and quadrupeds.

The first thing that attracted my notice more particularly to this subject, was finding that in all animals, whose stomachs are made up of a great variety of parts for the purpose of economizing the food, the colon has a greater extent of surface, and the course of the canal is so disposed, that its contents must be a long time in their passage through it. This circumstance led me to believe that the food, after the chyle is formed and separated from it, undergoes in the lower intestines some changes, by which a secondary kind of nourishment is extracted from it.

This opinion was much strengthened, by finding that the colon of the casuary from Java is only one foot long, and each of the cæca which are appendages to it, only six inches long, and a quarter of an inch in diameter, while the African ostrich has the colon forty-five feet, and each of the cæca two feet

nine inches in length, and at the widest part three inches in diameter: besides which, both the colon and cæca have very broad valvulæ conniventes not met with in the casuary from Java. This wonderful difference, for it is more than fifty to one, can only be explained by the luxuriancy of Java being so great, that this bird might destroy its health by over-feeding, had no guard been furnished by nature.

This guard is, the food passing through the intestines with so much facility, and in so short a time, that, however much the bird may eat, only the necessary quantity of nourishment is carried into the constitution; but in the African ostrich, the food is retained in the extensive colon till every thing nutritious is extracted. In all ruminating animals, the colon is of great length, is fixed in its course, which is very intricate, and varies in every different genus; so that we cannot doubt of some particular process being carried on in it.

The process which the contents of the colon undergo, is quite distinct from any thing carried on in the other intestines, since they entirely change their appearance and smell; and there is commonly a valve to prevent any part of them, even the gases evolved, from being carried up into the small intestines.

The peculiar smell of the fæces, which borders so closely on that from putrefaction, although by no means the same, led me to compare them with the animal matter buried in the earth, which is converted into adipocere: in both cases the substance is in the incipient state of putrefaction, but that process never completely takes place; it is excluded from the external air, is either under water, or within the reach of imbibing moisture; and there is no substance whatever, the chyle

excepted, which can better supply the waste produced by the actions of growth and muscular exertion, than animal fat.

The more I canvassed this new opinion, the greater number of circumstances in favour of it occurred to me; one of the strongest of which is, that there is no other mode I am acquainted with, by which animal fat can be formed. To this may be added the curious circumstance of the sleeping animals, which lay in so large a supply of it, in a short time, to serve for their winter's consumption, having a formation of the intestines almost peculiar to themselves, in which there is no valve to distinguish the colon, and no fixed course for that intestine; so that the contents pass along with more facility, and remain a shorter time in the canal, the food being sufficiently plentiful during the summer to compensate for this want of economy, by which the lower intestines receive more abundant supplies for the production of fat. These intestines remain empty during the sleeping season, so that no fat can be formed in that period.—With this very important information, thus procured, in support of my opinion, I have been led to prosecute this inquiry with increased ardour, and shall now bring forward the facts I have been able to ascertain in confirmation of my hypothesis. These I shall detail in the order in which they were acquired, thinking it better to lay before the Society the regular process of the investigation, than to grasp at once at the conclusions, which in the end of it I have felt myself authorized to draw.

I shall therefore begin by stating the circumstances under which adipocere is formed from animal matter, most nearly resembling those in which the contents of the lower intestines in living animals are placed; and this I shall do from facts,

entirely within my own knowledge, the specimens of the adipocere being now in my possession; and afterwards go on by bringing forward proofs that a substance similar to it is formed in the colon.

Mary Howard, aged forty-four, died on the 12th of May, 1790, and was buried in a grave ten feet deep at the east end of Shoreditch church-yard, ten feet to the east of the great common sewer, which runs north and south, and has always a current of water in it, the usual level of which is eight feet below the surface of the ground, and two feet above the level of the coffins in the graves. In August, 1811, the body was taken up with some others buried near it, for the purpose of building a vault, and the flesh in all of them was completely converted into adipocere or spermaceti. In Stowe's History of London, this part of Shoreditch is stated to be a morass, and since that time the ground has been raised eight feet. The clerk and the grave-digger observe, that at the full and new moon the water in the sewer rises two feet, and that at those times, there is water found in the graves, which at other times are dry.

The current of water, which passes through the colon, while the loculated lateral parts are full of solid matter, places the solid contents in somewhat similar circumstances to dead bodies in the banks of a common sewer.

The circumstance of ambergris, which contains sixty per cent. of fat, being found, in immense quantities, in the lower intestines of the spermaceti whales, and never higher up than seven feet from the anus, is an undeniable proof of fat being formed in the intestines; and, as the ambergris is only met with in whales out of health, it is most probably collected

there from the absorbents under the influence of disease, not acting so as to take it into the constitution.

Ambergris is found in lumps from fourteen to more than one hundred pounds each; it is not to be distinguished in its appearance from the fæces, but when exposed to the air, it grows hard: a lump has been found in the sea weighing one hundred and eighty-two pounds.*

In the human colon, solid masses of fat are sometimes met with in a diseased state of that canal, and are called *scybala;* these are in all respects similar to ambergris.

Concretions of olive oil and mucus found in the human intestines must be formed in the same way. A case of this kind was communicated to me by our associate Dr. BABINGTON in the following letter:

"My dear Sir,

17, Aldermanbury, Feb. 2, 1813.

"The following are the circumstances relating to the "change produced upon olive oil, by passing through the "stomach and intestines of the elderly person, whose case I "mentioned to you at the last meeting of our Animal Che-"mistry Society. The lady, in question, had for several years "past suffered from severe affections of the stomach, which, "from the attendant symptoms, were considered as occa-"sioned by the irritation of biliary concretions. Many reme-"dies having been resorted to without affording her other "than temporary benefit, she was advised to try the effects "of olive oil, taken to the quantity of two or three ounces at "a time, and to be repeated as circumstances might require. "From this she experienced almost immediate relief, and, on

* Vide Phil. Trans. 1783.

" the subsequent examination of what passed from the bowels, " globular concretions were uniformly observed, which by " the persons about her were considered as the gall-stones, " which had previously been productive of so much distress. " This lady having occasion some months since to visit her " friends in town, and a doubt having been suggested by one " of her medical attendants in the country, as to the nature of " the concretions in question, I was desirous, from the account " that I had received, to have an opportunity of determining " the point for myself; and therefore requested, that if the " pain should recur, and she should be under the necessity of " repeating her medicine, that the concretions, which had " been said always to pass from the bowels in consequence of " her so doing, might be reserved for my inspection. In a " few days I was summoned to make my propósed visit, and, " upon examining the substances collected, I found their ap- " pearance to be such as I have already described to you, " namely, that of distinct globules, varying in size from that " of a large pea to the bulk of a moderate grape, of a cream " colour, and slightly translucent, of sufficient consistence to " preserve their form, and to bear being cut by a knife, like " soft wax, but at the points of their contact disposed to co- " here. When exposed to heat, they readily melted, and then " at once exhibited their original oily character. The change, " which they have since experienced, has taken place in the " water in which they have been kept.

" I am, dear Sir,

" yours always, very faithfully,

(Signed) WM. BABINGTON."

To Sir EVERARD HOME, Bart.

Our associate, Mr. BRANDE, afterwards examined the substance, and made the following report upon it.

"The globules voided appeared to be composed of the "olive oil combined with mucus: the latter separated during "putrefaction, and the oil was evolved, apparently unaltered. "The relative proportion may be estimated at one-third ani- "mal matter, and two-thirds vegetable oil."

The following case, which was also communicated to me by Dr. BABINGTON, shews that fat is sometimes formed in the intestines, and detected passing off with the fæces.

ELIZABETH RYDER, four years and a half old, had been healthy for six months after her birth, when she became thin, had a sallow complexion, and was liable to jaundice. At a year and a half old, her belly was tumid, and she had great weakness in her back and limbs, for which complaints Dr. BABINGTON was first consulted. At three years old, her mother observed something come from her, as she walked across the room, which, when examined, was found to be fat in a liquid state, which concreted when cold. Ever since that time to the present, she has voided, at intervals of ten or fourteen days, the quantity of from one to three ounces, sometimes pure, at others mixed with fæces; when voided, it has an unusually yellow tinge, and is quite fluid like oil. Her appetite is good, as well as her spirits, and her flesh firm; her belly rather tumid, but not hard: she is subject to occasional griping: her urine natural, and she sleeps well. The specimen on the table was procured under circumstances which precluded all possibility of deception.

These facts, so strongly in favour of the opinion I had taken up, led me to devise in what way it might be put to the test

of experiment. I tried to extract fat from the contents of the colon in different parts of its course, but without success. Failing in this mode of obtaining any decisive conclusions, I was led to believe the cæca of birds more favourable for experiments on this subject, and had those of a duck examined by Mr. W. Brande after the bird had been seven days without an evacuation. This confined state of the bowels put the parts nearly under the same circumstances, as if they were in a diseased state. When the cæca were examined, they were found completely distended with fæces of the consistence of soft clay, so that, when the bags were laid open, the contents retained the same form. The intestine, immediately above the cæca, was empty, but the rectum was much distended; its contents were of a softer consistence than those of the cæca.

The following is Mr. Brande's report on this subject.

" The contents of the cæca were divided into two portions,
" of one drachm each, and comparative experiments were made
" with similar quantities of the contents of the rectum.

" *Exp.* 1. One drachm of the contents of the cæcum was
" completely immersed in half an ounce of water, and kept
" for seven days in a temperature varying from 40° to 60°.
" At the end of that time, warm water was poured upon it,
" but no appearance of fat could be perceived.

" *Exp.* 2. The same quantity of the contents of the cæcum
" was immersed in water containing one-fifteenth part of
" nitric acid, and kept under the same circumstances as in the
" former experiment. In seven days, warm water poured upon
" it separated a portion of oily matter, which concreted when
" cold, and appeared to be one-eighth of the whole mass.

" *Exp.* 3. Portions of the contents of the rectum were " treated in the same way, as in experiments 1 and 2. That " in water became putrid very rapidly, and shewed no ap- " pearance of fat. The other, in the diluted nitric acid, was " more dissolved than in Experiment 2. Considerable extri- " cations of gas took place, but there was no appearance of " fat."

From these experiments, we learn that the contents of the cæcum, being confined there for some days, are in a state readily to be converted into fat by nitric acid, while the contents of the rectum are not, probably from being too putrid.

While engaged in this enquiry, I received from Sir Joseph Banks the carcase of a wild swan, which the Hon. Mr. Pelham had shot in the neighbourhood of the Humber. On examining the cæca, their contents were found to be of a bright green colour. This led me to propose to Mr. Brande to ascertain by experiment, whether an admixture of bile had any effect upon the process of converting animal substance into fat. The following experiments were made by Mr. Brande upon this subject.

***Exp.* 1.** He took two portions of human muscle of the same size, and digested one of them in human bile, the other in water, both placed in the temperature of 100°.—In the first day the muscle in the bile underwent no change. On the second day it became soft in its texture, and had a slightly fetid smell. On the third day it became more fetid and yellow. On the fourth it had the smell of excrement, was flabby, very offensive, and fatty upon the surface. The portion of muscle, digested in water, had undergone no other change in

the four days, but becoming slightly putrid, and there was no appearance of fat whatever.

Exp. 2. A similar experiment to that with the human bile, was made with a small portion of beef, and ox's bile, and the results were exactly similar.

Exp. 3. The last experiment was repeated in the temperature of 60°. In four days the beef became slightly fetid, and of a yellow colour; in six days it became more fetid, but there was no appearance of fatty matter.

Exp. 4. A portion of beef cut into pieces was digested in ox's bile, at the temperature of 100°. At the end of the fourth day the putrefaction was more advanced than in Experiment 2; the beef was washed and heated upon paper, but no greasy stain was produced.

From these experiments we learn, that the bile has a power of converting animal substance into fat; and that the temperature of 100°, or nearly so, is necessary for that process. We learn also, that this change is produced just as putrefaction is beginning to take place, and if the substance goes rapidly into putrefaction, no fat is formed, and, what is deserving of observation, the peculiar smell belonging to fæces, so different from that of putrid matter, is produced at the time that fat is procured.

Having succeeded in changing animal matter into fat, by adding bile to it out of the body, I was desirous of ascertaining whether this process could be detected going on in the human intestines, and being in attendance upon a gentleman of an advanced age, who had been six days without an evacuation from the bowels, confined to bed by the gout, I did not let slip the opportunity of his having a very costive stool deeply

tinged with bile, to make the experiment. The excrement was put into water, and kept heated for three hours to a temperature of above 100°. When the water was allowed to cool, a film was formed upon the surface, which appeared to be of an oily nature, and Mr. BRANDE ascertained it to be so. The quantity was not great, but quite sufficient to ascertain the fact, and next day the fæces having subsided, the fatty film was much more conspicuous. In the Phil. Trans. for 1673, p. 6093, a case is stated of a person who laboured under an indisposition, attended with sickness and vomiting. In one attack of vomiting, he brought up matter resembling tallow, four pieces of which weighed half an ounce.

This process of forming fat in the lower intestines by means of bile, throws considerable light upon the nourishment derived from clysters, a fact well ascertained, but which could not be explained. It also accounts for the wasting of the body, which so invariably attends upon all complaints of the lower bowels. It accounts, too, for all the varieties in the turns of the colon, which we meet with in so great a degree in different animals. This property of the bile explains likewise the formation of fatty concretions in the gall-bladder, so commonly met with, and which, from these experiments, appear to be produced by the action of the bile on the mucus secreted in the gall-bladder: and it enables us to understand the following effects, which arose from the circumstance of no part of the bile passing into the intestines.

A child was born, at the full time, of the usual size, and lived for several months, but never appeared to increase in size, although it fed heartily, had regular stools, and the food seemed perfectly digested. There was no bile in the stools,

and the skin was of a dark yellowish brown. I saw the child while it was alive, and was struck with its want of growth, and its having no fat under the skin, which made it appear longer than new-born children generally are. Upon examining the body after death, the only mal-formation met with, was there being no gall-bladder, nor any duct leading from the liver into the duodenum.

From what happened in this case, a supply of fat appears necessary for growth, for the child was by no means wasted in its muscles, which it must have been had the constitution not been supplied with nourishment.

Animal fat has, I believe, hitherto been considered as a secretion, although there is no direct evidence in favour of such an opinion. It has nothing in common with the secretions; it is met with in all the interstices of the body; is very often quickly deposited, and in as short a time taken back into the constitution. In these respects it corresponds with the watery fluids, with which the body is supplied.

In a former communication respecting the stomachs of animals, I explained that water was taken up from the stomach by channels yet unknown, and carried into the circulation; from whence it is poured into all the cavities of the body, or thrown out altogether by the kidneys and glands of the skin.

On the present occasion, I hope that I have collected a sufficient body of evidence to prove, that fat is formed in the intestines, and from thence received into the circulation, and deposited in almost every part of the body. When there is a great demand for it, as in youth, for carrying on the growth of the body, it is laid immediately under the skin, or in the

neighbourhood of the abdomen: when not likely to be wanted, as in old age, it is deposited in the interstices of muscles, to make up in bulk for the wasting of the muscular fibres. There appear to be no direct channels by which any superabundance of it can be thrown out of the body, so that when the supply exceeds the consumption, its accumulation becomes a disease, and frequently a very distressing one.

XXII. *On the colouring Matter of the black Bronchial Glands and of the black Spots of the Lungs.* *By* George Pearson, *M. D. F. R. S.*

Read February 25, 1813.

In the adult human animal the glands designated *bronchial* are generally of a black or dark blue colour. These organs, which are allowed to be lymphatic glands, as is well known, are situated at the root of the lungs externally, within cellular membrane, near the bifurcated trachea; as well as internally on or near the large branches of the bronchi.

At the age of about twenty years the lungs have a mottled, or marbled appearance, from black and dark blue spots, lines, and points disseminated immediately under the transparent pulmonary pleura. As hath been repeatedly observed, the lungs generally become more dark coloured proportionately to their age. Accordingly at upwards of sixty-five or seventy years of life, they often appear almost uniformly black, from the number and congeries, or coalescence of the maculæ, points, and lines just mentioned. Throughout the whole interior substance of the lungs the black spots are seen in a great measure corresponding to the external appearance.

I do not find that any observations and experiments have been made to determine the nature or cause of the black colour above described of the pulmonary organs. It is true, a conjecture has been proposed, that sooty matter taken in with

the air may be the occasion of the colour of the lungs; and that the colour of the glands is occasioned by a peculiar secretion. But the former conjecture has been supposed to be satisfactorily refuted by the absence of the appearance in question among brute animals; as also by its presence in persons who breathe the air of the provinces at a great distance from towns, or places of great consumption of coal; and the latter conjecture is palpably erroneous, because the bronchial glands, of which I am speaking, are not organs of secretion, but of conveyance of lymph.

The course of investigation, in which I have long been engaged, to improve the pathology of pulmonary consumption, led me to some experiments and observations on the subject now stated, which I respectfully submit to the consideration of the Society.

After cutting away the cellular membrane surrounding the black glands, and washing them till the water was no longer coloured, I subjected them to examination.

1. On pressure between the fingers, to burst the investing coat, a black fluid issued which stained the skin, which rendered water black, and which did not alter in colour or apparently dissolve, even at a boiling temperature, either in water or in concentrated muriatic and nitric acids.

2. On breaking down the structure and triturating in a glass mortar a number of these glands with a small proportion of water, a thick black liquid was produced, which was decanted from off many membranous and fibrous masses. But after repeated affusions and trituration, I could not deprive these masses of their black colour, although the water was at last scarcely tinged: it was only by dissolution in caustic potash

lye, or in nitric and muriatic acids, that I could totally separate the black matter from the animal substance to which it seemingly adhered. After repose, a black sediment took place in the waters of elutriation, as well as from the alkaline, and the acid dissolution, which on decantation and evaporation to dryness, afforded the deposit in the state of a black powder.

The texture and proportion of the tingeing matter of the glands was very different in different subjects; whether the lungs, to which they belonged, were in a healthy or diseased condition. In persons of about eighteen to twenty years of age, some of the bronchial glands contained no tingeing black matter at all, but were of a reddish colour; others were streaked, or partially black; and others were quite black, or of a dark blue colour.

3. By boiling the black glands in lye of caustic potash their structure was destroyed, and a turbid black liquid was produced: from which, on standing during several days, a copious black sediment took place; but the liquid still remained black, after remaining at rest a month; much of the tingeing matter continuing suspended. By dilution with water, this matter deposited in a clear liquid.

4. By liquid muriatic acid, of the specific gravity 1,170, the bronchial glands were dissolved at a boiling temperature, affording a turbid black liquor; but on repose, an abundant black deposit took place from a clear yellow liquid, as well as a quantity of the same matter appearing on its surface. On separating this precipitate, and evaporating to dryness, it became a black powder.

5. Nitric acid liquid, of the specific gravity 1,500, most speedily dissolved the substances under examination, affording

a clear yellow liquid with abundance of black matter on its surface. The separation of this coal-like matter is effected most easily with this acid.

It may not be quite superfluous to mention, that from the bronchial glands, which were not at all coloured with black matter, but were merely red, no black matter was separable by the above mentioned acid, and alkaline solvents.

I next examined the black and blue colouring substance of the surface and interior parts of the lungs.

1. A portion of the lungs beautifully marked with a crowd of these coloured areolæ, spots, points, and lines, after being washed till it no longer imparted a bloody tinge to water, still retained these appearances undiminished. On pressure of these coloured parts between the fingers, the skin was sometimes stained of a black colour.

2. On treating the substance of the lungs, abounding in the above described black and blue parts, in the same manner as the bronchial glands just related, with water, with liquid caustic potash, with muriatic and with nitric acids, similar phenomena, with specially the separation of black matter, was observed. The proportion, however, of this black substance was very much smaller to the animal matter dissolved, than in the former cases.

3. To shew that the black matter is of the same kind in the differently figured black parts of the lungs, it may be worth while to relate a particular experiment.

A thin slice of about three inches square of the surface of lungs exhibiting black lines, maculæ, and points under the transparent pleura, was put into a vessel containing three ounces measure of liquid nitric acid. On this it floated, but

all of it was presently dissolved, except the coloured parts, which retained their respective forms till the vessel was shaken: then they were destroyed, and afforded a coating or stratum of black matter on the surface.

Having collected an adequate quantity of the colouring matter in a powdery state, for examination, I performed a number of experiments, but it seems needless to relate more than a few decisive ones.

1. A little of the well exsiccated black powder being sprinkled upon fused nitrate of potash, deflagration took place as with charcoal of wood, or with soot.

2. The same phenomenon occurred with melted oxymuriate, or chlorite of potash, but at a much lower temperature.

3. The deflagration with nitrate of potash, and also with chlorite of potash, was produced, in a suitable glass vessel, to collect the compounded gas; which was received into lime water, and found to be charcoal acid.

4. A little of the black powder was very easily ignited upon a plate of platina, and was speedily burnt off, smelling like burning animal matter, and leaving a minute residue, sometimes of reddish powder and at other times of white.

5. The powder was ignited in a green glass tube closed at one end, and kept red hot from ten to fifteen minutes, the open extremity being slightly stopt with a clay lute to prevent the admission of air. A white vapour with water was discharged, and a minute portion of animal empyreumatic oil was condensed in the upper part of the tube. There remained on cooling, a fine black powder, which in different trials lost $\frac{1}{7}$ to $\frac{1}{4}$; *i. e.* about $\frac{14}{100}$ to $\frac{25}{100}$ of the original weight of the substance.

6. On treating the coaly powder by exposing it to fire, and with the pneumatic apparatus, the products were always charcoal acid, hydro-carbonate gas, and much water, with generally a little empyreumatic oil, and sometimes a trace of prussic acid, leaving a residue varying in weight as just stated.

From the properties above manifested, I conceive I am entitled to declare the black matter, obtained from the bronchial glands, and from the lungs, to be animal charcoal in the uncombined state; *i. e.* not existing as a constituent ingredient of organized animal solids or fluids.

I mean by the term animal charcoal, what is popularly understood. Of course, I do not mean pure charcoal. Such a state of this substance cannot here be reasonably expected, either from a consideration of the state of it, as inspired from the atmosphere, or from its necessary impregnation with animal matter during its long residence in the lungs. I imagine, no person would hesitate to consider such a coaly substance as the present to be charcoal, if derived from other sources besides the animal economy; it being, as shewn by the preceding experiments, a black, tasteless, infusible powder, indissoluble in muriatic acid, nitric acids, and perhaps all common acids, except the sulphuric; affording as large a proportion of charcoal acid as animal and vegetable charcoal which has been exsiccated at the same temperature, and equally resisting fire in close vessels.

For the purposes of physiology, a few theoretical remarks may, perhaps, be useful. I think the charcoal, in the pulmonary organs, is introduced with the air in breathing. In the air, it is suspended in invisibly small particles, derived from the burning of coal, wood, and other inflammable materials in

common life. It is admitted, that the oxygen of atmospherical air passes through the pulmonary air vesicles, or cells, into the system of blood vessels; and it is not improbable, that through the same channel various matters contained in the air may be introduced. But it is highly reasonable to suppose, that the particles of charcoal should be retained in the minutest ramifications of the air tubes, or even in the air vesicles, under various circumstances, to produce the coloured appearances on the surface, and in the substance of the lungs, as above described. It must also be considered, that innumerable absorbent lymphatic vessels take their rise in the bronchial tubes: for the lungs are more richly stored with lymphatic vessels than any other organ, excepting the liver. When I compared the black lines and black net-like figures, many of them pentagonal, on the surface of the lungs, with the plates of the lymphatic vessels by CRUICKSHANK, MASCAGNI, and FYFFE, I found an exact resemblance. And when I found that these vessels contained charcoal, I judged that it was fair to infer, that the lymphatics of the lungs absorb a variety of very different substances, and especially this coaly matter, which they convey to the bronchial glands, and thus render them of a black, or dark blue colour. Hereafter, among other enquiries, the colour of the large trunks of the lymphatic vessels, just *before* they enter the bronchial glands, and just as they pass out of them, ought to be observed. Also the effect of the charcoal thus conveyed into the thoracic duct, or directly into the blood by the lymphatics from the black glands, is, I presume, worthy of attention.

According to this theory, we can account reasonably for the absence of the black colour of the bronchial glands, and of the

lungs in infants, in children, and even frequently in persons of eighteen or twenty years of age. For the same reason these appearances are seldom observed in any brute animal which has fallen under my observation. On a subject so novel, or at least so much neglected, as that on which I am writing, many facts are wanting to establish, demonstratively, any theory which can be proposed; but I know of none, at this time, which are at variance with that I have ventured to offer.

George-street, Hanover-square, June 13, 1813.

P. S. Since my last communication to the Society, I have had an opportunity of making some further observations and experiments on this subject.

The lungs of an infant, who lived two days, were obligingly sent to me by my friend Mr. C. M. CLARKE. I found the bronchial glands quite white, and the lungs appeared externally of an uniform reddish colour. The pulmonary organs of a girl of fifteen years old, I found rather thickly mottled, but the bronchial glands were only tinged on their surface, and did not yield one-fourth of a grain of coaly powder. In two other females also, aged nearly fifteen years, who died of pulmonary consumption, the lungs were not at all marbled, but some of the bronchial glands were tinged black, and others were white. The lungs of two men, the victims of pulmonary phthisis, at the age of twenty-one years, exhibited thinly, black spots, streaks, and areolæ, with many of the glands in question of a deep blue colour. A woman's lungs, thirty-one years of age, were found studded beautifully with black spots, and lines; the bronchial glands being all either blackish

or blue. In no instance have I observed the lungs and glands, here spoken of, so black, and from which I separated so much charcoal, as in those of a person forty-two years old, whose death was occasioned by most extensively diffused tubercles, many vomicæ, and a considerable condensation of the pulmonary organs. I now recollect, that this subject had been a Smoker of tobacco, generally several times, but always once a day, for perhaps more than twenty years. Future observations must determine more satisfactorily the state of the pulmonary organs, according to the impregnation of the air with sooty vapours. If, hereafter, it be shewn, that the lungs of persons living remote from sources of such vapours, are still greatly impregnated with coaly matter, the just conclusion can only be, that such matter is more extensively diffused through the atmosphere, than is apprehended. This being the fact, it would also afford a proof that it is only the invisibly small particles which are absorbed, for the larger particles remain unabsorbed, entangled in the mucus lining the air vessels, and never get farther, but are rejected from time to time by expectoration. Accordingly, in a morning, healthy people, after the night's rest, very commonly hawk up mucous matter of a bluish colour with black streaks, owing to charcoal; and persons in a diseased state, especially by great exertions in coughing, frequently expectorate matter spotted and streaked with black particles. The quantity of coaly matter in the pulmonary organs is not entirely according to the age, for I was disappointed, on finding the lungs and glands in a woman of seventy-five years of age, in London, not more deeply coloured, than is usual at the age of fifty. At present, I am unable to state any connection between certain diseases, and the presence of coaly matter.

Farther investigation has shewn, that this coaly matter does exist in domesticated brute animals; but as they die, or are killed generally before they attain to twenty years, or even fifteen years of age, the organs in question are seldom seen blackened. However, in diseased conditions, the cases are not according to this rule. With the approbation of Professor COLMAN and Mr. SEWEL, several of the worthy students of the Veterinary College have frequently obliged me by furnishing, for my examination, the lungs of horses and asses. In general, the bronchial glands were merely white, or reddish; but now and then they were partially black. In one instance of an ass, only six months old, these glands were black from coaly matter; but the lungs were uniformly red. The animal had died of peripneumony. In no instance have I seen, in any brute creature, the lungs marbled and streaked with black lines as in the human. There is seldom an opportunity of inspecting horses which die from their natural age, viz. of thirty to forty years: for I am informed they mostly die or are killed in London before they are fifteen or sixteen years old. I have not seen coaly matter in the lungs, or glands of the ox kind, sheep, and hogs. The black appearances produced by distended blood-vessels and by ecchymosis, should be recollected, to avoid the error of ascribing them to charcoal. The absence of this matter in human creatures, at the ages just mentioned, when animals are slaughtered or die, affords a proof, although not a decisive one, that the exemption is more reasonably ascribable to the circumstance of time, and living in the open air, than to the peculiarity of the economy of each species of live being. Consistently with this observation, in the instance of a cat, known to have lived in

Mr. Thomas's family, at least eighteen years, the bronchial glands were quite black from coaly matter, and the lungs were uniformly red; but in all other examinations of much younger cats, I found these glands either white or red.

The blackness of the lungs from charcoal remains, although hæmorrhage to occasion death has occurred. It is not removable by ablution, or maceration in water, nor by acids, nor alkalies, nor by the early stages of putrefaction. I have not met with a similar coaly substance in any parts of the animal economy, except the lungs. The glands of the meso-colon are sometimes black, similarly to the bronchial; but the colour soon disappears on immersion in nitric or muriatic acids, no charcoal being separable. The black, or more truly the dark brown tingeing liquid of the sepia, I have ascertained by experiments, does not contain uncombined charcoal; this matter existing there only as a constituent ingredient of animal matter.

As I have represented, it is conceived that the coaly matter is very slowly absorbed by the mouths of the lymphatic vessels in the innumerable air tubes and cells.

To determine whether or not this matter exists in these lymphatic vessels, and is the occasion of the black maculæ, streaks, and areolæ, or marbled appearance of the surface of the lungs, I entreated Mr. Wharrie, of St. George's Hospital, whom I knew to be a skilful anatomist, to inject these vessels with quicksilver. In some trials, the injection passed without interruption, in the usual manner; but in others it was apparently obstructed, by meeting with the black lines on the surface. Mr. George Ewbank also, at my desire, very dexterously dissected out about one inch in length of one of these

black lines, supposed to be a lymphatic vessel. Being put into a glass capsule, full of nitric acid, the black line immediately was contracted in all dimensions; but it retained its form after digestion, for several days, at a high temperature: afterwards on gently shaking the capsule, the black line was broken into a number of indissoluble particles. In the interior of the lungs, it is not unusual to see black spots in the middle of tubercles, although these substances consist apparently of self-coagulated lymph probably secreted in the cellular substance, and therefore very likely to envelope the coaly matter in the air tubes by the coalescence of numerous minute tubercles.

It has been objected, that the nitric acid may develope the constituent or combined ingredient charcoal of all animal substances; and consequently no proof will be thereby afforded of this matter being extraneous; but on many trials, I have never by this means obtained charcoal from any animal mucilage.

I have no reason to believe, that any of the coaly matter under investigation, is dissolved by this acid, for on distilling a pint measure of it, from ten grains of the black powder of the bronchial glands, there was no sensible diminution of it, whether it was so treated before ignition, or subsequently: on evaporation to dryness, nitric acid, in which the coaly matter had been boiled, afforded no black sediment. Hence, I conceive, that this menstruum may be employed to determine, more accurately and speedily, the proportion of the coaly matter, than any other agent. Sulphuric acid does dissolve a certain portion of this charcoal, affording a transparent liquid, even on dilution with water.

XXIII. *Experiments on the Alcohol of Sulphur, or Sulphuret of Carbon. By* J. Berzelius, *M. D. F. R. S. Professor of Chemistry at Stockholm; and* Alexander Marcet, *M. D. F. R. S. one of the Physicians to Guy's Hospital.*

Read May 13, 1813.

There has been, of late years, much discussion respecting the nature of a singular oily liquid, which was first noticed by Mr. Lampadius,* who procured it by distillation from a mixture of pyrites and charcoal, and gave it the name of *alcohol of sulphur*, on account of its very great volatility.

Lampadius considered this liquid as a compound of sulphur and hydrogen; but Messrs. Clement and Desormes,† who obtained the same substance by subliming sulphur through red hot charcoal, were led by their researches to conclude, that the alcohol of sulphur was a combination of sulphur and charcoal, and that hydrogen was not one of its constituent principles.

Doubts, however, were entertained respecting the chemical nature of this compound. Mr. Berthollet believed it to be a triple combination of sulphur, charcoal, and hydrogen.‡ Messrs. Vauquelin and Robiquet,§ from a joint enquiry on the subject, considered it as a binary compound of sulphur and

* Crell's Annals, 1796. II.

† Annales de Chimie, An. X. Vol. XLII. p. 121.

‡ Ibid. p. 286.

§ Ibid. Vol. LXI. p. 145.

hydrogen; and the late Mr. BERTHOLLET, jun.* who resumed the subject, with much apparent accuracy, was induced, from his experiments, to adopt the last mentioned opinion, that the alcohol of sulphur was a compound of sulphur and hydrogen, and that no carbon entered into its formation.

Sir HUMPHRY DAVY, in the course of his brilliant career of chemical discovery, has repeatedly noticed this singular substance;† but it does not appear that he ever examined it minutely, and if he gave the preference to the opinion of the younger Mr. BERTHOLLET, respecting its chemical nature, it was principally from the circumstance of its having apparently yielded sulphuretted hydrogen by the agency of the Voltaic electricity, and sulphureous and sulphuric acid by combustion in oxygen.

These varieties of opinions, amongst such respectable authorities, having left on our minds much doubt and uncertainty with regard to the real nature of this compound, we undertook to examine it conjointly; in hopes that we might succeed in ascertaining its composition with a greater degree of certainty than our predecessors had done, and perhaps also in discovering the exact proportions of the elements of which it is composed, a circumstance which, in the present state of chemical science, would add considerable interest to an enquiry of this kind.‡

* Mémoires d'Arcueil, Vol. I.; and Annales de Chimie, Vol. LXI. p. 127.

† Phil. Trans. 1809, page 464; and Elements of Chemical Philosophy, page 283 and 310.

‡ This enquiry was undertaken in London, in the months of July, August, and September, during Mr. BERZELIUS's stay in this country; and the leading points of the analysis were then ascertained, and mentioned to our chemical friends. Some parts of the work, however, and in particular the precise determination of proportions, remained to be completed; and this has been accomplished by Professor BERZELIUS

Whilst we were finishing this paper, and arranging our materials, in order to present them to the Royal Society, the Annales de Chimie, for the month of September last, reached this country, by which it appears that the alcohol of sulphur has recently been again submitted to examination in France, by Mr. CLUZEL, who supposed it to be a compound of sulphur, carbon, hydrogen, and azote.* But Messrs. BERTHOLLET, THENARD, and VAUQUELIN, the reporters of these experiments, have repeated them, and by a process of their own, quite different from the methods which we have employed, these celebrated chemists have revived the opinion of CLEMENT and DESORMES, that the alcohol of sulphur is a compound of sulphur and carbon, in the proportion of about eighty-five parts of the former to fifteen parts of the latter, no hydrogen entering into its composition. How near this conclusion agrees with our own, will be seen in the following pages.

Although some of the experiments on this substance which we are going to relate, have already been performed by others, yet as we have, in several instances, obtained different results, and as any considerable omission would interrupt the thread of our narrative, we shall beg leave to lay before the Society a complete, though concise, account of our analysis.

We shall, for the sake of arrangement, class the particulars of our enquiry under four heads: viz. 1. *Preparation and general*

since his return to Stockholm. There are also some important collateral objects, which are almost entirely his own, and which, together with his remarks on determinate proportions, have been added to the paper in the form of an appendix. (Note of Dr. MARCET.)

* It appears that Mr. CLUZEL considered the sulphur in this oil, as being in a deoxydated state, an idea which had some years ago occurred to one of us, and also to Sir H. DAVY. See Phil. Trans. for 1809, p. 465.

properties of the Alcohol of Sulphur. 2. *Experiments to ascertain whether Hydrogen be present in the Alcohol of Sulphur.* 3. *Experiments to ascertain the presence of Carbon.* 4. *Experiments to ascertain the proportions of the elements of the Alcohol of Sulphur.* And the Appendix will contain various elucidations and collateral objects.

§ 1. *Preparation and general Properties of the Alcohol of Sulphur.*

We prepared this substance according to the method pointed out by CLEMENT and DESORMES, that is, by slowly volatilizing sulphur through red hot charcoal in a porcelain tube, and condensing in water the oily liquid which is thus formed. The details of the process have so often been described elsewhere, that it would be superfluous to repeat them here. The fluid thus procured is of a pale yellow colour; being extremely volatile, it produces a remarkable degree of cold during its evaporation, and deposits in a crystalline form, some sulphur which it held in solution. The quantity of sulphur thus dissolved in the oily liquor, varies according to the circumstances of the process by which it is obtained;* and its specific gravity, before it has been rectified, must, of course, be liable to corresponding variations.†

In order to obtain the alcohol of sulphur perfectly pure, it is only necessary to distill it at a very gentle heat, not exceeding 100° or 110°; and some dry muriate of lime may be put into the retort, in order to obtain the liquor perfectly free from moisture. The fluid which comes over is quite pure, and some sulphur remains in the retort.

* The alcohol of sulphur can dissolve as much as $\frac{1}{3}$ of its weight of sulphur.

† We ascertained the specific gravity of only one specimen of the impure oily liquor, which proved to be 1,321.

The alcohol of sulphur thus prepared has the following properties: it is eminently transparent, and perfectly colourless. Sometimes, immediately after distillation, the oily liquid appears somewhat opaque and milky; but the next day the liquor is found perfectly clear, the milky appearance having spontaneously disappeared. It has an acrid, pungent, and somewhat aromatic taste; its smell is nauseous and fetid, though distinctly differing from that of sulphuretted hydrogen. Its specific weight is 1,272; its refractive power, as ascertained by Dr. WOLLASTON, is 1,645. Its expansive force (the height of the barometer being thirty inches, and the temperature 53,5° FAHRENHEIT) is equal to the pressure of 7,36 inches of mercury; so that air, to which it is admitted, will dilate about one-fourth of its volume. It boils briskly under the common atmospheric pressure at a temperature between 105° and 110°.* It does not congeal at a temperature as low as 60° below zero of FAHRENHEIT's scale. It is highly inflammable, and takes fire at a temperature scarcely exceeding that at which mercury boils; it burns with a bluish flame, emitting copious fumes of sulphureous acid. If a long glass tube, open at both ends, be held over the flame, care being taken to keep the tube quite cold, no moisture whatever is deposited on its internal surface.

This oily liquid readily dissolves in alcohol and ether,† though not in all proportions; and if it contain sulphur in solution, the

* The volatility of this liquid is very remarkable; it exceeds considerably that of ether; and in some experiments, tried since this paper was presented to the Society, on its power of producing cold by evaporation, some unexpected results have been obtained, which may, perhaps, become the subject of some future communication.

† Ether can dissolve about three times its own bulk of the oily liquor before any separation or turbidness takes place.

latter is rapidly precipitated in spicular crystals. The spirituous solution is rendered turbid, and the oily liquor precipitated by the addition of water. The alcohol of sulphur is not soluble in water, though it imparts to it some of its odour; this appears to be owing to its impregnating the atmospheric air contained in the water, rather than the water itself. It readily mixes, and perfectly incorporates with either the fixed or volatile oils; but if it hold sulphur in solution, the latter separates in a crystalline form. It dissolves camphor very rapidly, and forms with it perfectly transparent solutions. When kept for some time under water, the latter being in contact with atmospheric air, neither the air, nor the oily liquid, suffer any alteration. On being heated in contact with potassium, the alcohol of sulphur suffers no change or decomposition, when in its liquid state; but if it be brought to the state of vapour, by the exclusion of the air, and heat applied, the potassium becomes ignited in the vapour, and emits a reddish flame. During this species of combustion, a blackish film appears upon the potassium, (as happens when potassium is burnt in carbonic acid gas), and on introducing water, a greenish solution is obtained, which contains a quantity of carbonaceous matter, and has the smell and other properties of hepar sulphuris. Neither mercury nor amalgams of silver, or of lead, are at all acted upon by this liquor, if it has been carefully distilled; but if it contain any sulphur in solution, the amalgam blackens, and sulphurets of silver, or of lead, are produced, after which the liquor remains pure, and unsusceptible of further alteration. Mercury heated to ebullition in an atmosphere of this oil, comes over unaltered, and without the liquor undergoing any change. The alkalis dissolve it entirely, though but very slowly. None of the

acids exert any sensible action upon this liquor, except the nitro-muriatic acid, and oxy-muriatic gas in a humid state. If a globule of the alcohol of sulphur be brought into contact, under water, with a globule of the oily fulminating compound, lately described to this Society by Sir H. DAVY, the two globules remain in contact, side by side, without mixing, and without exerting any action upon each other; but when stirred together, they incorporate, forming a homogeneous amber-coloured globule, which does not detonate, even when exposed to ignition; and if olive oil be brought into contact with the two oils thus mixed, no detonation or other obvious effect takes place, a remarkable circumstance, of which advantage might be taken to attempt the analysis of the fulminating substance. The detonation of the fulminating compound with phosphorus, is also prevented by the presence of alcohol of sulphur; but if the quantity of the detonating compound preponderates over that of either the olive oil or phosphorus, the mixture inflames, though without detonation.*

§ 2. *Experiments to ascertain whether Hydrogen be present in the Alcohol of Sulphur.*

1. We introduced into Volta's eudiometrical tube, over mercury, some pure oxygen gas, with one drop of the oily liquor. After letting these stand together for a few minutes, an electrical charge was passed through the tube, which produced a vivid explosion. The gas was first reduced to between one-

* These experiments were tried in the presence of one of us, by Mr. WILSON, Assistant to the Chemical Lectures of GUY's Hospital, who, conjointly with two other gentlemen, has lately published in NICHOLSON's Journal, many curious facts on this extraordinary compound.

fifth and one-sixth of its volume, a circumstance which appeared to be owing to the destruction of the volatilized portion of the liquor; for the gas soon afterwards recovered its former volume. On examining the superior part of the tube, we perceived traces of a condensed liquid. Whether this liquid was concentrated sulphuric acid, or simply a small portion of the oily liquor, remained to be determined. We repeated the experiment in the following manner.

A quantity of the oily liquor was suffered to evaporate in oxygen gas of known purity, and previously dried with muriate of lime. Some of this gas was introduced into the eudiometer, and exploded by the electrical spark. It lost between one-fifth and one-sixth of its volume; but we perceived, even then, traces of a condensed liquid in the superior part of the eudiometer. This liquid, in about a quarter of an hour, became opaque, and ultimately formed white specks, which we found to be sulphate of mercury. No oxygen gas remained in the tube after the explosion; but the gaseous residue consisted of sulphureous acid gas, and, as we shall see afterwards, of the carbonic acid, and the carbonic oxyd gases. This experiment having still left undetermined, whether the vestige of liquid we had observed, was or was not to be ascribed to the formation of water, we tried to decide this point by means of oxymuriatic gas, in the following manner.*

2. We caused a succession of bubbles of oxymuriatic gas,

* In relating the experiments in which oxymuriatic gas (or chlorine) is concerned, we have used the old nomenclature, and have explained the phenomena according to the notions, respecting the nature of this agent, which prevailed previous to Sir H. Davy's ingenious experiments and speculations on the subject. But those who consider his views, as sufficiently established to supersede entirely the old hypothesis, may easily adapt to our statements the language which belongs to the new doctrine.

previously dried by muriate of lime, to pass through a portion of the alcohol of sulphur, and afterwards through a quantity of distilled water, over which it was collected. The oily liquid suffered no other change than that of acquiring an orange hue. After an hour and a half the process was stopped, and the liquor was found to have absorbed a considerable quantity of the gas, which had imparted to it a peculiar and extremely strong odour. Most of the gas, however, had passed through the water, in which it had deposited a portion of the oily liquid unaltered. The water had acquired a peculiar smell, and contained, after the expulsion of the oxymuriatic gas, a little muriatic acid, with a vestige of sulphuric acid. Here again it remained doubtful, whether the production of muriatic acid was owing to the alcohol of sulphur containing hydrogen, which, by uniting with the oxygen of the oxymuriatic acid, had formed water; or whether this appearance of minute quantities of muriatic and sulphuric acid, might not be explained in some other manner. It will hereafter be seen how the last conjecture was verified; but in the mean time it was sufficiently shewn, by the experiment just related, that if the alcohol of sulphur really contain hydrogen, it must be in very small quantity, and probably from some accidental circumstance, since the greater part of the liquor remained undecompounded, and with no other alteration than the absorption of the oxymuriatic gas. In the course of a few days, however, the oil gradually lost the smell of this gas, and acquired that of the muriated sulphur, described by Dr. THOMSON, (Sir H. DAVY's sulphurane). The liquor thus treated, on being exposed to the action of water, lost its colour, and resumed its original

characters; and this was accomplished more quickly, if the water contained some alkaline substance.

3. Into a glass receiver, full of oxymuriatic gas, we immersed some of the oily liquor previously ignited in the air; it was instantly extinguished, and appeared to undergo no change, except that of absorbing a little oxymuriatic acid, and thereby acquiring a yellow colour.

4. A portion of the oily liquor, in the state of vapour, was caused to pass through liquified muriate of silver heated to a cherry red, and to condense again into a small receiver artificially cooled; neither the liquor, nor the muriate of silver, were altered by that operation, nor did the air contained in the apparatus appear, on examination, to contain the smallest quantity of acid. This shewed that the liquor did not contain any hydrogen, since if it had, the muriate of silver would have been decomposed, the hydrogen uniting with the oxygen to form water, so as to generate muriatic acid gas (which may be considered as a muriate of water, or more correctly a muriate of hydrogen), whilst some sulphuret of silver would have been produced.

5. Though the result just related appeared sufficiently conclusive, we thought it desirable to have it confirmed by some other mode of proceeding. With this view, we heated to incipient redness, in glass tubes, various metallic oxyds, such as red oxyd of iron, black oxyd of manganese, oxyd of tin; and we caused quantities of the oily liquor in vapour to pass through these ignited oxyds. The liquor was, by that means, entirely decomposed; the metallic oxyds were converted into sulphurets, and the gases acquired a strong smell of sulphureous

acid. But we could not, in any of our experiments, detect the least production of water, although our apparatus was so devised, as to render the smallest quantity of water conspicuous, and although we burnt, in some instances, as much as fifty or sixty grains of the liquor.

From all these experiments, we think ourselves warranted in concluding, that *the alcohol of sulphur contains no hydrogen.*

§ III. *Experiments to ascertain the presence of Carbon in the Alcohol of Sulphur.*

1. The gaseous residue obtained from the combustion of the vapour of the alcohol of sulphur in oxygen gas, by means of VOLTA's eudiometer (§ II. 1.), being put in contact with water, was in a great degree absorbed, and the water acquired the taste and smell of sulphureous acid. The remaining gas being agitated with lime water was partly absorbed, and produced a precipitate of carbonate of lime. The unabsorbed portion being mixed with oxygen gas, and the electrical spark passed through the mixed gases, they detonated, and the remaining elastic fluid was found to have again acquired by this detonation the property of rendering lime water turbid, and of forming carbonate of lime. The residue of the first detonation was therefore gaseous oxyd of carbon.

2. We introduced into a glass receiver, filled with pure oxygen gas, and inverted over mercury, a small glass capsule full of the oily substance, which we kindled in the air before we plunged it into the gas. It continued to burn, and we were not a little surprised to find, that the heat of this combustion was sufficiently intense to melt a pretty strong platina wire, by which the capsule was suspended, so that it fell with its

contents upon the surface of the mercury, where the oily liquor continued to burn until it was all consumed. Water being then introduced, an absorption of gas took place, and the water became sulphureous. The unabsorbed portion of gas being introduced into lime water, rendered it turbid, and formed a quantity of carbonate of lime, which was sufficient to be carefully examined and accurately estimated.

3. We caused some alcohol of sulphur to dissolve in barytic water, in a well stoppered bottle. This solution proceeded very slowly, and was only completed at the end of three weeks. The barytic water had assumed a yellowish colour, and had deposited a white precipitate, which being well washed, and treated with liquid sulphureous acid, dissolved in it with effervescence. It was therefore carbonate of barytes.

4. A similar experiment being tried with lime water, an analogous result was obtained.

It follows from these experiments, that *the alcohol of sulphur contains carbon*, and therefore that this body, the nature of which has been the subject of so much doubt and speculation, is a true Sulphuret of Carbon, a name by which, in compliance to the received chemical nomenclature, we shall henceforth usually designate it in the course of this paper.

§ IV. *Experiments to determine the Proportions of Sulphur and Carbon, in the Sulphuret of Carbon.*

In attempting to analyse, with accuracy, the sulphuret of carbon, considerable difficulties occur from the great volatility of that compound, and from the inconsiderable action which most chemical agents exert upon it.

1. We first tried to ascertain the proportion of its constitu-

ent parts, by exploding it, in a state of vapour, with oxygen gas, in Volta's eudiometer; but this method presented insurmountable obstacles, and in particular that of our not being able to determine, with accuracy, the quantity of the oily substance actually decomposed by that process.

2. On the other hand, on attempting simply to burn in oxygen gas the sulphuret of carbon in its liquid state, the necessity of previously kindling it in the air, in order to avoid explosions, prevented our being able to ascertain, with accuracy, the quantity of the oily substance submitted to the experiment.

3. We also attempted the oxydation of the sulphuret of carbon by nitro-muriatic acid; but we obtained by this method, as we shall see afterwards, results and combinations which were quite different from those which we had expected.*

4 We then had recourse to the action of alkaline substances on the sulphuret of carbon, with a view to produce an alkaline sulphuret, and afterwards to convert this into a sulphate by means of nitro-muriatic acid. The sulphuric acid, precipitated by a barytic salt, was ultimately expected to indicate the quantity of sulphur contained in the oily compound. In order to try this method, we exposed portions of the sulphuret of carbon to the action of caustic potash, ammonia, barytic water, &c. and when the solutions appeared to be completed, we tried the effect of acids upon them; these separated from them a soft, reddish, glutinous substance, which, when exposed to distillation, yielded, first, a portion of the oily liquor undecomposed, and afterwards sulphur; thus shewing that the

* A new and singular compound was obtained from the long continued action of nitro-muriatic acid on the alcohol of sulphur, the history and composition of which will be detailed in the Appendix.

whole of the sulphuret of carbon had not been decomposed. A black powder remained in the retort, which was found to be charcoal, and afforded us a new and unequivocal proof of the presence of carbon in the oily substance.

This imperfect decomposition of the sulphuret of carbon in alkaline solutions, was found an insurmountable objection to this mode of analysis; and induced us to try the method of distilling the sulphuret through ignited metallic oxyds.

5. For this purpose, we introduced a glass tube (BC), in an inclined direction, through a small stove (fig. 1, A), the

Fig 1.

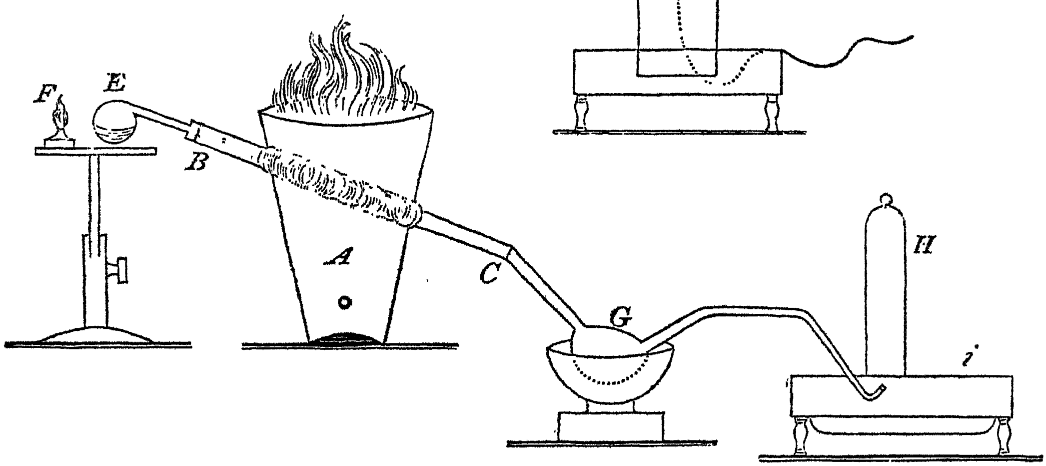
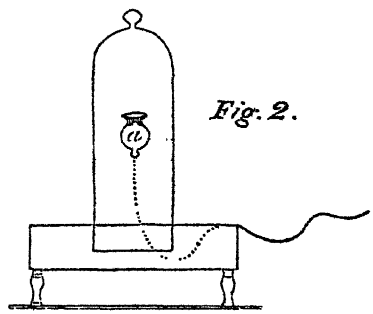


Fig. 2.



tube being protected by lute, and filled with red oxyd of iron coarsely pulverized. The sulphuret of carbon, after being accurately weighed, was enclosed in a little retort (E), which we hermetically sealed to the upper extremity of the tube (B). To the other extremity (C), we adapted another glass tube

(CGI), terminating in a mercurial pneumatic apparatus (H), and having a bulb or expansion (in G), at about half way between its extremities, which bulb was cooled down to about 15° FAHRENHEIT, and kept at that temperature during the experiment by means of a mixture of ice and salt. The tube in the stove (BC) being now heated to redness, and an extremely gentle degree of heat applied to the retort, the oily liquor distilled slowly through the red hot oxyd of iron, and the whole of the experiment succeeded in a most satisfactory manner; for we found that the joints of the apparatus had remained perfectly tight during the process, and every vestige of the sulphuret of carbon had been decomposed.* We could not perceive in the bulb the least appearance of moisture.

6. The oxyd of iron (the whole weight of which was about ten times that of the oil decomposed), was partly converted into sulphuret of iron, having a yellow colour and a brilliant metallic lustre. This sulphuret was dissolved by nitro-muriatic acid, and the oxyd of iron was separated by ammonia. The filtered liquid, being neutralized by muriatic acid, was precipitated by muriate of barytes, and the precipitate was well washed, and heated to redness.

7. During the process of decomposition above related, the mixture of sulphureous acid gas and carbonic acid gas had collected in the receiver (H) placed on the extremity of the

* The process was so slowly conducted, that it required six hours and a half for the decomposition of about fifteen grains of the oil. The heat applied to the retort was merely that which radiated from a small lamp (F), placed at some distance from it, a screen being interposed between the furnace and the retort. In several previous trials, we had failed from not being sufficiently cautious and gradual in the mode of warming the retort.

tube. In order to separate these two gases from each other, we made use of the brown oxyd of lead, a substance which one of us had often employed for the purpose of combining with, and separating from other bodies, liquid sulphureous acid, with which that oxyd forms a neutral sulphate. The same method, to our great satisfaction, succeeded perfectly with the gaseous sulphureous acid. After a contact of about one hour, between the brown oxyd of lead and our mixed gases, the whole of the sulphureous acid gas was so completely absorbed, that the carbonic acid gas which remained in the vessel, had not the least sulphureous smell, and yet the carbonic acid itself remained perfectly unaltered in its bulk, after continuing in contact with the oxyd of lead for the space of three days.*

8. The mode in which the respective weight of the two acids was ascertained was this; the oxyd of lead was placed in a small glass capsule (*a*, fig. 2), the orifice of which was carefully secured, by means of a thin piece of glove-leather, against the admission of quicksilver. A very thin flexible metallic wire was fastened to the capsule, so as to enable the operator to raise it through the mercury in the receiver containing the gases, as is expressed in the figure (fig. 2). After continuing the contact for several hours, and when the absorption had

* The brown oxyd of lead, used in our experiments, was recently prepared, and had been long digested in weak nitric acid. In wiping this oxyd, and pressing the moisture out of it, we found it expedient to reduce it to the state of small laminæ, as in this form it is more easily penetrated by the acid gas than in the state of powder. It may be proper to remark, that when, for the sake of trial, gaseous mixtures, consisting of sulphureous acid gas and sulphuret of carbon in vapour, were used, the latter, as the absorption of the acid gas proceeded, was deposited in its liquid state upon the oxyd of lead.

ceased for a considerable time, the capsule was withdrawn, and another similar capsule filled with dry caustic potash (hydrate of potash), was introduced in a similar manner. The remaining gas (with the exception of a small residue),* was immediately absorbed. Both capsules, after wiping off minute particles of quicksilver which adhered to their surface, were carefully weighed and were found to have undergone an increase of weight, which, of course, expressed the quantity of acid they had respectively absorbed. By combining these results with the examination above described (§ IV. 6.), of the sulphuret of iron found in the tube, the proportions of sulphur and carbon were deduced. Two such experiments were made which yielded very similar results; and by a mode of computation, which will be detailed in the Appendix, we were led to conclude that the alcohol of sulphur, or sulphuret of carbon consists of

Sulphur -	84,83 or	100,00
Carbon - -	15,17	17,89.

If these proportions be compared with the numbers 13 and 5, which are assumed by Mr. DALTON as representing the respective weights of a particle of sulphur and a particle of carbon; or with those of 30 and 11,4, which are considered by Sir HUMPHRY DAVY as representing the same substances, we shall find that either of these modes of computation, as well as that detailed in the Appendix, make the above-mentioned proportions of sulphur and carbon, in the alcohol of sulphur, correspond very nearly to two atoms or portions of sulphur, to one of carbon; a result which is perfectly agreeable to the doctrine of determinate proportions. And as the

* This small residue corresponded almost exactly to the quantity of air expelled from the tube in which the decomposition had been performed.

sulphur and carbon recovered by our analysis, were precisely equal in weight to the sulphuret of carbon subjected to examination, we are the more confident in believing that it does not contain any other element.

APPENDIX BY PROFESSOR BERZELIUS.

A. *Particulars respecting the Mode in which the Proportions of Sulphur and Carbon, in the Alcohol of Sulphur, were established.*

Two analytical experiments were made in succession, in the manner described in the paper (§ IV.), with the following results.

Exp. 1. 1,05 parts * of sulphuret of carbon produced 0,24 parts of sulphureous acid gas, and 0,59 parts of carbonic acid. The sulphuret of iron found in the tube was dissolved in nitro-muriatic acid; the oxyd of iron was precipitated from this solution by caustic ammonia in excess, and the filtered liquor was neutralized by muriatic acid. From this solution, muriate of barytes precipitated a quantity of sulphate of barytes, which, after ignition, weighed 5,6. From Mr. T. DE SAUSSURE's experiments, as well as from inference from the doctrine of determinate proportions, 100 parts of carbonic acid contain from 27 to 27,1 parts of carbon. Therefore the above mentioned 0,59 parts of carbonic acid contain 0,1593 of carbon. From the experiments on the composition of sulphate of barytes, published by myself, 100 parts of that salt contain 13,66 of

* The weights represented by these numbers were *grammes*; so that the quantity of sulphuret of carbon employed in this experiment, was 1,05 gramme, corresponding to about 15$\frac{1}{4}$ English grains.

sulphur; therefore the 5,6 parts of sulphate of barytes, are equivalent to 0,765 of sulphur, which, added to the 0,1224 parts of sulphur contained in the 0,24 parts of sulphureous acid gas, make 88,74 parts of sulphur. The sum total of sulphur and carbon is, therefore, $88{,}74 + 15{,}93 = 104{,}67$. But the quantity of the sulphur of carbon was 105 parts; consequently there is a loss of $\frac{1}{3}$ per cent. From this experiment therefore, the sulphuret of carbon is composed of 15,7 parts of carbon to 84,83 parts of sulphur.

Exp. 2. 1,175 parts of sulphuret of carbon produced 0,66 of carbonic acid gas, = 0,1782 parts (which is equivalent to 15,167 per cent.) of carbon; and 0,271 parts of sulphureous acid gas = 0,138 of sulphur. In the superior extremity of the glass tube which contained the oxyd of iron, there was found, in that part of the tube which projected out of the stove, a small portion of sublimed sulphur weighing 0,029. The sulphuret of iron found in the tube afforded, by the mode of estimation above related, 6,06 parts of ignited sulphate of barytes = 0,829 of sulphur; therefore the sum total of sulphur is $0{,}829 + 0{,}29 + 0{,}138 = 0{,}996$ parts of sulphur, which added to 0,1782 parts of carbon, makes 1,1742 parts, or 0,0008 less than the weight of the sulphuret of carbon employed in the analysis.

B. *Comparison of the above Proportions of Sulphur and Carbon, in the Sulphuret of Carbon, with those which might be inferred from the laws of determinate Proportions.*

The law respecting the combination of combustible bodies between themselves, is that, *when two such bodies unite, the proportion in which this combination takes place is such, that if they*

be oxydated to a certain degree, they will either absorb an equal quantity of oxygen, or the one will absorb two, three, four, &c. times as much oxygen as the other. As to the metallic sulphurets in general, those which are called sulphurets *at the minimum*, are so composed that the sulphur requires twice as much oxygen in order to become sulphureous acid, or three times as much in order to become sulphuric acid, as the metallic body requires to become an oxyd or saline basis; and it is on that account, that the sulphurets in minimum produce, by their oxydation, neutral sulphites, or sulphates. Now, by comparing the quantities of oxygen required to convert the sulphur into sulphureous acid, and the carbon into carbonic acid, it appears that the first quantity is twice as great as the latter. The small difference is to be ascribed to the impossibility of obtaining perfectly accurate results in analytical processes so complicated as this; and therefore the sulphuret of carbon may be considered as constituted according to the same law as the other sulphurets; so that the sulphur which it contains requires twice as much oxygen to become sulphureous acid, as the carbon requires to become carbonic acid. Admitting, therefore, that the above views of the composition of those two acids are correct, the sulphuret of carbon would consist of 15,47 parts of carbon, to 84,53 parts of sulphur; and in that case the result of our analysis of the alcohol of sulphur would only differ by $\frac{1}{3}$ per cent. from the proportions obtained by that mode of estimation.

C. *Observations on the atomic doctrine of chemical Combination.*

Mr. DALTON has lately proposed a mode of viewing the subject of determinate proportions, which is distinguished by

its extreme simplicity. From that ingenious mode of expressing combinations, the sulphuret of carbon ought to consist of two atoms of sulphur to one of carbon, since, according to that system, both the sulphureous and carbonic acids are composed of two atoms of oxygen to one of radical. But it is probable, that the gaseous oxyd of carbon consists of two atoms of carbon to one of oxygen, because the quantity of carbon required to convert carbonic acid into carbonic oxyd, exactly doubles the volume of the acid. In this case, and if, according to Mr. DALTON's supposition, sulphureous acid consists of two atoms of oxygen to one of sulphur, the alcohol of sulphur would be, like all the other sulphurets at the minimum, composed of one atom of sulphur to one of carbon.

Sir H. DAVY, in his Elements of Chemical Philosophy, adopts, like Mr. DALTON, the idea that sulphureous acid gas consists of one portion of sulphur to two of oxygen; and agreeably to this, he supposes for instance that the sulphurets of copper, or of silver, are composed of one portion of sulphur to one of the metal. But on the other hand, he considers other sulphurets, such as those of antimony, of iron, and of lead, as consisting of two portions of sulphur to one of the metal, and the sulphuret of zinc would, according to his views, be composed of two portions of the metal to one of sulphur. Yet, notwithstanding this great diversity in the estimation of proportions, all these sulphurets are so constituted that, if the compound be fully oxydated, it remains in a state of perfect saturation. The cause of these apparent inconsistencies seems to be, that the supposed atom, or ultimate particle, or that which is to be regarded as the single portion of a body, requires to be fixed according to some determinate law, before Mr. DALTON's

ingenious method of expressing the proportions in which bodies combine, can be applied with certainty and precision.*

D. *Experiments on the Combination of the Sulphuret of Carbon, with the Alkalies, the Earths, and the metallic Oxyds.*

The following experiments will shew in an unequivocal manner, that the sulphuret of carbon is capable of combining with saline bases. These combinations constitute a new class of bodies, for which there is no name in our present chemical nomenclature. I shall propose to call them *carbosulphurets*, a name quite consonant with the principles of the received chemical nomenclature.

The unavoidable presence of water, in either caustic potash or soda, induced me to try, in the first instance, the action of ammoniacal gas on the sulphuret of carbon.

Carbosulphuret of Ammonia. Some ammoniacal gas, and some liquid sulphuret of carbon were successively introduced into a receiver filled with mercury, the sulphuret being enclosed in a small glass bulb having an open orifice. The first effect of this contact was a dilatation of the gas; the surface of the sulphuret soon covered itself with a pulverulent straw-coloured substance, as if a portion of sulphur had been precipitated. After a few hours, the gaseous mixture had sensibly

* It appears to me that the best way to form a system of definite proportions, and to make it harmonize with the general views of chemistry, would be to take oxygen as the base of the scale, because most chemical combinations turn upon the proportion of that ingredient; and as gaseous bodies unite in equal or multiple volumes, I would consider as *the atom* of any other gas, the proportional weight of an equal measure of that gas.

diminished in bulk, and as this diminution proceeded, the gas gradually deposited on the surface of the glass a saline yellowish substance, which did not exhibit, even with the aid of a microscope, the least appearance of crystallization. The mercury having at last filled the receiver, an additional portion of ammoniacal gas was introduced, and this was repeated till no further absorption took place. The whole of the sulphuret of carbon was then found converted into the yellowish uncrystallized matter just described. This substance had a strong smell of ammonia, and was so deliquescent that it could not be transferred from one vessel into another, without undergoing an obvious alteration. The solution of this substance in water is first red, but it very soon passes to a deep orange colour, shewing that it undergoes a partial decomposition; and if it be distilled in its solid, though humid state, it sublimes and deposits small shining crystals of hydrosulphuret of ammonia; whilst, on the contrary, if it be heated in the same vessel in which it is formed, that is, without any access of air or moisture, the carbosulphuret of ammonia sublimes unchanged, from one part of the vessel to the other, and no vestige of hydrosulphuret is perceived. It appears therefore, that the sulphuret of carbon can enter into combination with pure ammonia without depositing its carbon; but if moisture or air be admitted, an alkaline hydrosulphuret, or sulphuret is formed, and carbonic acid is generated.

Carbosulphuret of lime. If some pure quick lime be heated in a glass tube, by means of a lamp, and some sulphuret of carbon in vapour be made to pass through the heated earth, the latter becomes ignited at the moment the vapour comes into contact with it, and this ignition continues till the earth is

saturated. During this process, none of the sulphuret of carbon escapes, the whole of it being absorbed by the lime. The earth, at its surface, is found yellowish, owing to the formation of a little sulphuret of lime, but this appearance ceases on removing the surface, shewing that it arises from the contact of air. The earthy mass is tasteless, when first applied to the tongue, but a bitter taste, with a smell of sulphuretted hydrogen are soon perceived. It is no longer susceptible of being heated or dissolved by water. If digested with water in close vessels, a solution of hydrosulphuret of lime is obtained, and the undissolved portion is mostly found to be carbonate of lime.

Carbosulphurets of barytes and strontian may be produced in a similar manner, and are found to possess analogous properties.

There can be no doubt but that the two fixed alkalies, in a state of perfect dryness, would form, with the sulphuret of carbon, *carbosulphurets of potash and soda*, quite analogous to those just described; but as these alkalies cannot be obtained in an anhydral state, I could only examine their action on the sulphuret of carbon when in a state of solution. The general result of these trials was, that if a quantity of the sulphuret of carbon be long digested, at a very gentle heat, with a solution of caustic potash, the sulphuret of carbon is decomposed, and the mixture resolves itself into a hydrosulphuret, and carbosulphuret, and a carbonate of potash.

In the same manner, if sulphuret of carbon be long digested with liquid caustic ammonia, it dissolves very slowly, forming an orange coloured solution analogous to that in caustic potash just described.

I tried also to produce *carbosulphurets of metallic oxyds.* The process simply consisted in precipitating metallic solutions, by solutions of the sulphuret of carbon in caustic potash. The precipitates which were thus obtained, had characters sufficiently distinct to shew that they were chiefly metallic carbosulphurets; but as the presence of a hydrosulphuret, and of carbonate of potash, in the solutions used for these precipitations, necessarily interfered with the distinctness of the results, I shall not trouble the Society with a detail of these experiments.

E. *Experiments to determine the nature of a particular Substance, produced by the action of nitro-muriatic Acid, on the Sulphuret of Carbon.*

We have already alluded, Dr. MARCET and myself, in the course of our paper, to a new and singular substance, which we produced by the long continued action of nitro-muriatic acid on the sulphuret of carbon. This compound was discovered in the following manner.

A portion of sulphuret of carbon was exposed to the action of a mixture of fuming nitric acid with concentrated muriatic acid. The acid instantly acquired a peculiar odour resembling that of muriated sulphur. Upon trying to promote the combination by applying heat, the sulphuret of carbon was expelled in the form of vapour, which obliged us to leave the mixture to its own spontaneous action, under the temperature of the atmosphere, which was then at least 70°.* The sulphuret of carbon soon passed to a reddish orange colour, which it communicated to the acid; and during this change,

* In August, 1812.

nitrous gas, with a strong smell of muriated sulphur, was slowly evolved. After the space of a week, the liquor began to cover itself with a white crystalline substance, losing at the same time its colour. On stirring the mixture, these crystals fell to the bottom, where they were immediately dissolved by the remaining sulphuret of carbon. This, however, in proportion as it saturated itself with the new formed substance, gradually lost its colour, acquired a greater consistence, and at last, at the end of three weeks, it was entirely converted into a solid white crystalline body, having the appearance of camphor.

This substance being separated from the acid, and washed with cold water, presented the following characters: it was colourless; its smell resembled both that of muriated sulphur and of oxyd of osmium; its taste was both acrid and acid; it was very volatile, melted at a gentle heat, and sublimed without residue. In fact, this body very closely resembles camphor in its external properties. It is insoluble in water, but readily dissolves in alcohol and ether, from which it is precipitated by water; it is also soluble in the oils, whether fixed or volatile, with which it forms transparent solutions. The spirituous solution of this substance has a peculiar, disagreeable, and very acid taste. It reddens litmus paper, and dissolves zinc with the disengagement of an extremely fetid gas. From this solution, a spirituous liquor is obtained by distillation, which possesses the same smell, and leaves a residue of muriate and sulphate of zinc. When a pure solution of the peculiar substance in alcohol is submitted to distillation, the products are, first, some sulphureous acid gas, and then alcohol strongly impregnated with muriatic ether; after which the air of the

vessel is found to contain portions of sulphureous and carbonic acid. The peculiar substance, in its dry state, does not alter litmus paper; but it reddens it strongly if the paper be moistened. When exposed to the action of boiling water, this body volatilizes through the water, but the remaining liquor contains some muriatic and a little sulphuric acid. Water, therefore, has the power of decomposing, though very slowly, this substance. Indeed if it be left a long time in contact with a small proportion of water, the liquor becomes strongly acid. Liquid caustic potash assisted by heat, dissolves the peculiar substance without any disengagement of gas. This solution is colourless; when neutralized by sulphuric acid no precipitation takes place from it, which shews that the alkali decomposes the peculiar body. Sulphuric acid, however, produces a slight effervescence, and the liquor exhales a smell of sulphureous acid. On adding to it a solution of sulphate of silver, muriated silver is precipitated,

These results tend to shew, that the substance in question contains sulphur and carbon in combination with oxygen, that is in the state of sulphureous and carbonic acid; for otherwise, some carbon would have been precipitated, or some sulphuret or hydrosulphuret of potash would have been formed.

And again, a portion of the problematic substance being sublimed through ignited lime in a glass tube, it was absorbed by the lime without any vestige of sulphuret of lime being formed, or any carbon deposited. And a similar experiment being tried with ignited metallic iron, instead of lime, some muriate of iron and a sulphuret of oxyd of iron* were formed, and carbonic acid gas was disengaged. This last

* *Sulphure d'oxidule de fer.*

experiment in particular shewed clearly that the substance in question contains carbonic acid.

The existence of three acids united in this compound, being thus ascertained, their respective proportions remained to be determined. It was by an experiment similar to the last described, that this object was obtained. The outline of this analysis, a minute detail of which would unnecessarily prolong this communication, was as follows:

A portion of the compound, after being weighed with great accuracy, was sublimed through a tube containing some very fine spiral iron wire in a state of ignition, the weight of which was three times that of the compound submitted to analysis. A decomposition took place, the products of which were, as in the experiment above related, muriate of iron, sulphuret of oxyd of iron, and a mixture of the carbonic oxyd and acid gases. These being carefully examined by appropriate reagents,* so as to form an estimate of the quantities of the muriatic, sulphureous, and carbonic acids contained in the peculiar compound, the following ultimate result, respecting the nature and proportions of its constituent parts, was obtained, viz.

Muriatic acid - -	48,74
Sulphureous acid -	29,63
Carbonic acid (and loss)	21,63
	100,00

* The proportion of muriatic acid was estimated by dissolving the muriate of iron in water, and precipitating the acid by nitrate of silver; that of the sulphureous acid was ascertained by treating the sulphuret of iron with nitro-muriatic acid, and precipitating the sulphuric acid formed by muriate of barytes: from the known relation between the sulphureous and sulphuric acid, the quantity of the former was easily deduced. And, lastly, the proportion of carbonic acid was gained by reducing the gaseous mixture to the state of carbonic acid, and absorbing this by caustic potash.

From the mode of computation adopted by Mr. DALTON, this triple acid would be composed of two atoms of muriatic acid, to one of the sulphureous, and one of the carbonic acid. It is a remarkable circumstance that the proportions of sulphur and carbon which prevail in the sulphuret of carbon, no longer obtain in this compound; one of the atoms of sulphur being expelled during the formation of the triple acid, and converted into sulphuric acid, which is found in the nitro-muriatic liquor.

The reason why the combination in question does not take place on burning the sulphuret of carbon in oxymuriatic gas, is that this gas does not yield a sufficient quantity of oxygen to acidify the sulphur and carbon, which are to unite with the muriatic acid, so that either nitric acid must be added, or a portion of muriatic acid must be detached from the oxymuriatic gas, by the agency of water, or of some other oxydated body.

It will be necessary to give a name to this triple acid. The combination of the fluoric with the boracic acid has been called *fluo-boric acid;* but the word *fluo* does not express the degree of acidification of the radical, which would be of great advantage in systematic language. In the Essay on the Latin Chemical Nomenclature, which I published some time ago, I proposed the name of *acidum boracico-fluoricum.* Upon the same principle, the combination, of the muriatic and carbonic acid, discovered by Mr. J. DAVY, would be named *acidum carbonico-muriaticum;* the combination of the sulphureous and muriatic acid (should such a compound be discovered), would be called *acidum sulphuroso-muriaticum;* and our triple acid would be *acidum muriaticum sulphuroso-carbonicum*, a name inconvenient by its length, but perfectly consonant to the principles of Chemical Nomenclature.

XXIV. *On the Means of procuring a steady Light in Coal Mines without the danger of Explosion. By* William Reid Clanny, *M. D. of Sunderland. Communicated by* William Allen, *Esq. F. R. S.*

Read May 20, 1813.

The many dreadful explosions of fire-damp, or inflammable air, which have occurred in the extensive and well regulated coal mines of this district, in the course of the nine years during which I have resided in the county of Durham, have often excited my most serious attention; and latterly these explosions have caused the death of so many industrious people, that no individual, possessed of common humanity, can look on the subject with indifference.

Though the improved methods of ventilation have been attended by many solid advantages to the proprietors of coal-mines, it is nevertheless worthy of remark, that the increased frequency of explosions clearly demonstrate, that ventilation, in this case, has been no preventive.

Ventilation undoubtedly supplies atmospheric air; but it cannot obviate those inundations of inflammable air, (if I may be permitted the expression,) which, rushing from the old workings and caverns of the coal mine, overwhelm every thing before them. It is evident that ventilation, even in its improved state, has afforded no relief whatever; and here the apparatus, which, *in the first instance*, I have the

honour to lay before the Royal Society, will be found to afford a good light, unaccompanied by danger.

It very frequently happens that accumulations of carburetted hydrogen gas, mixed with atmospheric air, take place in the wastes, or old workings of the coal mines, and though much precaution is used for keeping this inflammable air confined to its proper places by means of partitions and folding-doors, nevertheless when, by carelessness or accident, this air comes into contact with any ignited substance, an explosion generally takes place.

These explosions happen when the pit-men are occupied in hewing out the coal at the *workings*, should they chance to open a cavern of unmixed carburetted hydrogen gas. This gas for the most part being pent up in a condensed state, rushes forth from a chasm, and forming what is locally denominated a *blower*, it suddenly mixes with the atmospheric air of the mine, and surrounding the lights of the pit-men, an explosion follows, commensurate with the quantity of hydrogen gas, which is frequently very considerable.

It will be unnecessary to detail the phenomena of an explosion of inflammable air, as they are already sufficiently known; but I hope it will not be unacceptable to the Society to record a few of the more considerable explosions, which have occurred in the course of the last seven years, in this district alone, independently of those which have taken place in other parts of the kingdom within the same time. In the summer of 1805, an explosion happened at Hebburn colliery, by which thirty-two pit-men were killed, who left wives and children in a destitute state, to the number of one hundred and five. About the same time, a colliery at Oxclose blew up, by

which, I understand, thirty-eight men perished, leaving eighteen widows, and seventy children unprovided for.

Soon after this melancholy catastrophe, ten men perished at Killingworth by an explosion. And about the same time, seven men were instantly killed, and several severely wounded by an explosion at Fenton Park colliery. On the 25th of May, 1812, the colliery at Felling exploded, by which ninety-two persons were instantly destroyed, leaving forty-one widows, and one hundred and thirty-three children to the protection of the public.

And, upon the 10th of October last, the Harrington Mill pit exploded, by which twenty-three people were killed, and many others severely wounded and scorched.

Thus, in the short space of seven years, upwards of two hundred pit-men were deprived, most suddenly, of their mortal existence, besides a great many wounded; and upwards of three hundred women and children were left in a state of the greatest poverty and distress.

The great danger of these explosions, even when every precaution has been taken, is manifest by their frequency, and indeed it may be expected, that an explosion will take place by means of a lighted candle the instant that the hydrogen gas amounts to one-twelfth part of the atmospheric air present, and that a similar effect will follow at all proportions from one-sixth to one-twelfth.

When ventilation, by the methods in general use, is found insufficient to carry off the fire-damp, as it arises in coal mines, large pumps are employed at the top of the shaft for that purpose, which are worked by steam engines. So frequent and instantaneous are the changes in the proportions of inflam-

mable air, from accidental circumstances, that it would be impossible at all times to ascertain, by a chemical process, at all parts of the mine, when danger is impending, for frequently the greatest differences of proportions exist at the same time, in different parts of such extensive works as coal mines. In fact, the miners know, from the appearance of the light of their candles, when the proportion of hydrogen gas is such, as to threaten an explosion; hence they carefully watch each other's candles, that they may desist in time, and escape instant destruction.

The excavations of coal mines are much greater than they are generally supposed to be: in some collieries they are continued for many miles, forming numerous windings and turnings, along which the pit-men have frequently to walk for forty or fifty minutes before they arrive at the *workings*, during which time, as well as when at work, they have no direct communication with the surface of the earth, but are entirely at the mercy of their greatest enemy, the inflammable air. This circumstance first impressed me with the idea that the light, by which the pit-men were to work, might be insulated. I was well aware that no preparation of phosphorus could supply a sufficient light for the purpose; an observation equally applicable to the miserable scintillations of *steel mills* (as they are termed), which have often exploded the inflammable air of the coal mines.

I find it needful here to remark, that, as far as applies to myself, the idea of insulating the light, and also the plan which I have adopted for carrying this idea into effect, by the construction of the apparatus or lamp, are perfectly original. This lamp may be managed with the greatest ease by any

boy of common understanding. It is so strong, that should large pieces of coal fall upon it, they cannot in the least injure it. Nor is there any chance of its being upset by any accident, as it may be worked at the very bottom of the mine; and it is likewise conveniently portable.

The combustion of the candle, within the lamp, is supported by the ordinary atmospheric air of the coal mine, which is supplied by a pair of common bellows through a stratum of water below the candle; at the same time a portion of the air already in the lamp, is driven through another stratum of water above the candle, and thus the air supplied may explode within the body of the lamp, without communicating the effect to the air in the mine, however highly it might be charged with carburetted hydrogen gas.

The moment the air enters into the lamp, it comes in contact with the candle, and consequently, upon all occasions, a *small portion only* of the air can be exploded, instead of the whole contents of the lamp; by these means several obvious advantages are secured. The air passing in a brisk current from below upward, close by the candle, carries the snuff with it, so that the light is always clear and steady.

I may also remark that *wherever* a person can exist from a sufficiency of atmospheric air, this lamp will afford a safe and abundant light, from one candle only, for the space of five hours at least. This lamp will, in all probability, be found very useful in the powder magazines of ships of war, and of forts, as also in those places where gunpowder is manufactured; but this observation is merely thrown out for the opinion of those who are more conversant upon such subjects.

Fig. 1.

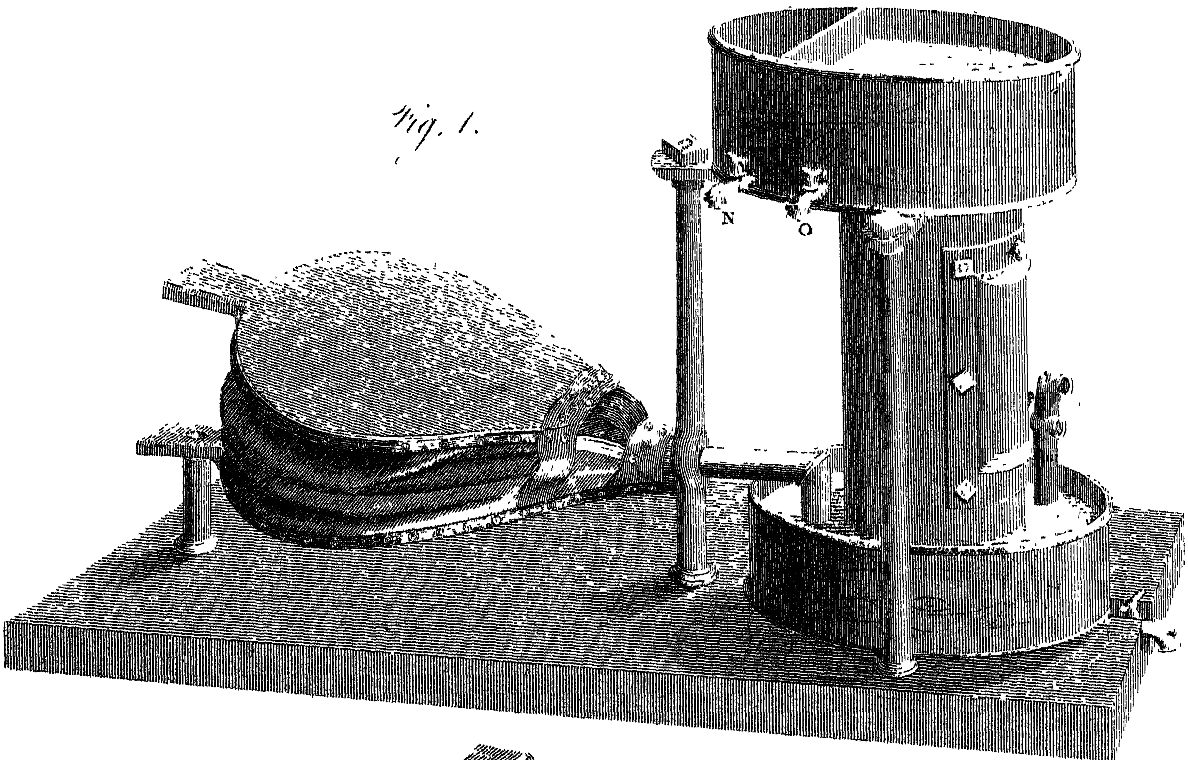
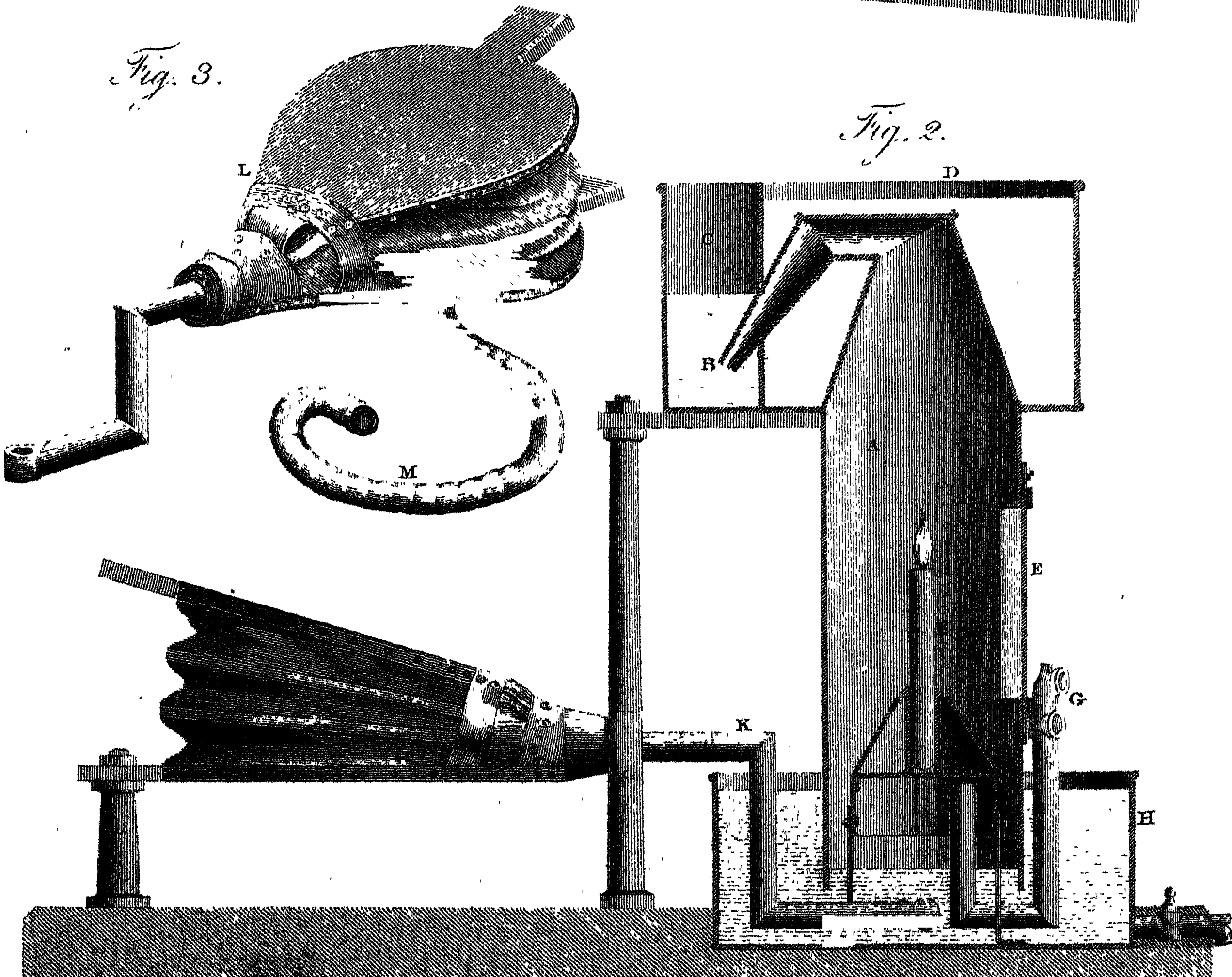


Fig. 3.

Fig. 2.



P. Basire sc.

Description of the Lamp.

(See Plate XV.)

Fig. 1. describes the lamp as it appears when ready for use.

Fig. 2. A section of the lamp.

A. The body of the lamp constructed of copper or japanned iron, terminating in

B. A conical tube, which carries off the air (deprived of part of its oxygen by combustion) through the water in the cistern C.

D. Is a cistern containing water, in order to prevent the lamp from being over-heated.

E. The window of the lamp made of very thick glass.

F. The candle supported upon a tin stand.

G. A tube furnished with a cock, in order to bring the water to a level within the lamp.

H. A cistern containing water, which may be drawn off by the cock I.

K. A tube from the bellows which delivers air for the supply of the lamp, through the water in the cistern H.

Fig. 3. L. Spare bellows with an elastic tube M which may be adapted to a tube conveying pure atmospherical air, or to a gasometer. Forty gallons of atmospherical air will be sufficient to keep the lamp burning for one hour.

Fig. 1. N and O, two cocks to draw off the water from the cisterns C and D.

XXV. *On the Light of the Cassegrainian Telescope, compared with that of the Gregorian. By Captain* Henry Kater, *Brigade-Major. Communicated by the Right Hon. Sir* Joseph Banks, *Bart. K. B. P. R. S.*

Read May 27, 1813.

The Cassegrainian telescope from its first invention to the present time, has generally been considered to be merely the Gregorian disguised, and to possess no other advantages over it than the capability of being made shorter with the same magnifying power. This opinion, joined to the inconvenience of its inverting the object, has caused it to be thrown aside, perhaps too hastily, and without a sufficient examination of its properties.

As the experiments which I am about to detail may possibly lead to important conclusions, I shall perhaps be pardoned if I relate the circumstances which induced me to engage in them.

A self-taught artist of the name of Crickmore, who resides at Ipswich, had by exclusive attention to the subject, brought the Gregorian telescope to a degree of perfection surpassing any thing of the kind I have ever yet met with. Some months since, in the course of his experiments, he first completed a Cassegrainian telescope of one foot in length, and on viewing Jupiter with it, with a power of about 100, I was instantly struck with the brightness of the image, far exceeding what might have been expected from the aperture; but I supposed

this to have been a deception arising from the specula being more exquisitely figured than usual, which, producing greater distinctness, occasioned the idea of superior light. A short time after this, the same artist received an order for another telescope, which, from the success that had attended his recent efforts, he recommended to be of the Cassegrainian form. The aperture was five inches, the length thirty inches, and with a power of near 400 the image was so perfectly distinct and luminous, that I could no longer hesitate to conclude that, from some unknown cause, the Cassegrainian telescope actually possessed far more light than the Gregorian, and I waited most anxiously for an opportunity of verifying this, and determining the difference by experiment.

Such an opportunity soon presented itself, and under circumstances peculiarly favourable, as another excellent telescope of the Cassegrainian form was made, and I was fortunate enough to procure a Gregorian made by Mr. CRICKMORE some time before. The mirrors of both these telescopes were cast at the same time, and from the same pattern, so that no difference of light could arise from any difference in the composition of the metal. The magnifying power of both instruments was ascertained by experiment to be very nearly equal; but the excess was rather on the side of the Cassegrainian.

The telescopes being placed side by side, were directed to a printed card, at the distance of fifty yards; and on viewing it, the far superior brightness of the image in the Cassegrainian was strikingly apparent. Having prepared a circular piece of paste-board to close the end of the Cassegrainian telescope, I drew a number of concentric circles on it, at the distance of the twentieth of an inch from each other. The paste-board

was then placed in the end of the tube, and an aperture was made, which was enlarged by cutting out one circle after another till the card appeared equally bright through both telescopes, and of this, the eye judges most accurately.

The following measures were then taken.

Cassegrainian Telescope.	Inch.	Gregorian Telescope.	Inch.
Diameter of the circular opening in the paste-board - -	2, 70	Diameter of the large mirror - -	3, 90
Diameter of the back of the small mirror -	1, 09	Diameter of the back of the small mirror -	1, 00
Length of the arm -	0,805	Length of the arm -	1, 45
Thickness - -	0,200	Thickness - -	0, 20

From the above measures the following calculations were made.

Cassegrainian Telescope.		Inch.	Gregorian Telescope.		Inch.
Area of the circular opening in the paste-board		5,726	Area of the large mirror		11,946
Area of the back of the small mirror to be deducted -	0,933		Area of the back of the small mirror to be deducted -	0,785	
Area of the arm to be deducted - -	0,161		Area of the arm to be deducted -	0,290	
		1,094			1,075
Area of the portion of the mirror exposed to the light - -		4,632	Area of the portion of the mirror exposed to the light - -		10,871

From this experiment it appears, that the light in both telescopes was equal when the area of the aperture of the Cassegrainian, was to that of the Gregorian, as 4,632 to 10,871. Now the increase of light being (under similar circumstances) directly as the area of the aperture, it follows that if the aperture of the Cassegrainian be made equal to that of the Gregorian, the light in favour of the former will be as 10,871 to 4,632, or in the surprising proportion of 7 to 3 nearly.

A difference of such magnitude could not be admitted but with extreme caution, particularly as the Gregorian telescope had been made some time, and its mirrors might therefore be supposed not to possess so high a polish, as those of the Cassegrainian which had been recently finished; but I was soon enabled to pursue the subject, as a Gregorian telescope was made by Mr. CRICKMORE fully equal, if not superior, to any he had before constructed; the mirrors were of an exquisite polish. The Cassegrainian, used in this experiment, was the one I formerly mentioned, the aperture of which was five inches, and the length thirty inches. It had not been carefully preserved, and the large mirror had lost somewhat of its original polish. All circumstances being thus in favour of the Gregorian, a paste-board circle was prepared, and the experiment conducted as before. When the images of the card were equally bright, the following measures were taken.

Cassegrainian Telescope.

	Inch.
Diameter of the circular opening in the paste-board - -	3, 50
Diameter of the back of the small mirror -	1,375
Length of the arm -	1,063
Thickness of the arm	0, 20

Gregorian Telescope.

	Inch.
Diameter of the large mirror - -	3, 95
Diameter of the back of the small mirror -	0, 95
Length of the arm -	1, 50
Thickness of the arm	0,175
Length of a bar containing the adjustment -	0, 70
Its width - -	0, 15
Diameter of three semi-circles used as rests for the great mirror -	0,375

From the above measures the following calculations were made.

Cassegrainian Telescope.

		Inch.
Area of the circular opening in the paste-board		9,621
Area of the back of the small mirror to be deducted -	1,485	
Area of the arm to be deducted -	0,213	
		1,698
Area of the portion of the mirror exposed to the light - -		7,923

Gregorian Telescope.

		Inch.
Area of the large mirror		12,254
Area of the back of the small mirror to be deducted -	0,709	
Area of the arm to be deducted -	0,263	
Area of the bar containing the adjustment to be deducted	0,105	
Area of the three semi-circles to be deducted - -	0,166	
		1,243
Area of the portion of the mirror exposed to the light - -		11,011

The magnifying power having been determined (by experiment) to be 188 in the Cassegrainian, and 182 in the Gregorian, the expression for the relative quantity of light becomes $\frac{11,011}{\overline{182}|^2}$ to $\frac{7,923}{\overline{188}|^2}$ or as 332 to 224, being nearly as 3 to 2.

In the first experiment, the advantages of polish were, *perhaps*, on the side of the Cassegrainian telescope; in the last, they were *much* in favour of the Gregorian; a mean therefore of both results may probably be considered as approaching the truth, and the light of a telescope of the Cassegrainian construction, may be taken, to that of a Gregorian of the same aperture and power, as about 60 to 33.

A fact so new, naturally leads the mind to hazard a conjecture as to the cause. In the Gregorian telescope a column of light from a point of the object, is received on the large mirror, and reflected in a cone of rays, the vertex of which is its focus, where an image is formed. Here all these rays meet in a single *point*, and crossing each other, fall on the small concave mirror whence they are again reflected, and form another image near the eye. Now, if light be supposed to consist of particles of matter, is it not possible that these particles, crossing in the same point, may interfere with each other? or, when thus forced within a certain distance of each other, may not a power of repulsion exist, which would occasion many of them to be dissipated? In the Cassegrainian telescope the rays reflected from the great mirror are received by the small convex mirror *before* they arrive at their focus, and are consequently reflected back without having crossed as in the Gregorian. The conclusion then seems to be, that wherever an image is formed, much light is lost, and this con-

clusion perhaps derives additional force from a circumstance noticed in most elementary works on optics, viz. that the satellites of Jupiter and his belts, may be distinctly seen with a Galilean telescope, whilst with an astronomical telescope of an equal aperture and power, they remain invisible.

Ipswich, 22d April, 1813.

XXVI. *Additional Observations on the Effects of Magnesia in preventing an increased Formation of Uric Acid; with Remarks on the Influence of Acids upon the Composition of the Urine. By* William Thomas Brande, *Esq. F. R. S. Prof. Chem. R. I. Communicated by the Society for improving Animal Chemistry.*

Read June 3, 1813.

In a paper which I had the honour of laying before this Society, about three years ago, and which is published in the Philosophical Transactions*, some cases are related, illustrating the effects of magnesia in preventing an increased formation of uric acid, and some experiments are detailed, instituted with a view to discover its mode of action.

Since that period many opportunities have occurred, both to Sir Everard Home and myself, of confirming its efficacy upon a more extended scale, and of ascertaining the efficient treatment of those cases in which magnesia is ineffectual, and in which it has even been found to aggravate the complaint.

To bring forward additional evidence in favour of the use of magnesia, and to distinguish the cases in which its use is indicated, from those where it is improper or hurtful, are the principal objects of the present communication, and will be considered in the two following sections.

* For 1813, p. 106.

Section I.

The following is the case of a gentleman who suffered from a calculous complaint, during which he was accidentally induced to employ magnesia, the effects of which he has thus described.

Case 1. About twenty-seven years ago, I felt a pain in one of my kidnies, particularly when in bed, which continued to increase during six months. I had likewise an occasional sympathetic pain in the testicles, and violent and excruciating pains in the left kidney now became frequent. These attacks were sometimes brought on by stooping to take up something; but at other times without any apparent cause. They lasted from twelve to twenty-four hours, and I obtained some relief from the application of warm flannels; but they always left me languid and relaxed.

On the fourth attack I consulted a physician, who imagined that my complaint had been induced by drinking cyder, in which I had formerly indulged. He ordered me weak Hollands and water for common drink, and prescribed the lixivium of tartar to be taken in broth. This medicine was persevered in for some time; but I found it gradually weaken my stomach, and impair my digestive powers.

About nine months after my first attack in the kidney, I walked from Hampstead to London after dinner, and on the following day, I clearly felt something pass from the kidney to the bladder, and suspected what it was. I took about a pint of Hollands and water, and on attempting shortly afterwards to void my urine, found that the passage was blocked up, but had scarcely time to consider of my situation before the

obstruction moved forwards to within an inch of the extremity of the urethra: it remained there till the following evening, when, by the help of a small pair of watchmaker's forceps, I succeeded in extracting a stone, which was the source of the mischief.

It was jagged and rough, and of a deep brick-red colour. I afterwards voided a considerable quantity of red crystalline sand.

My physician, who was apprehensive of a return of the disorder, desired me to purchase of CADELL, an anonymous pamphlet upon the Stone and Gravel, and to observe the rules there laid down. This treatise particularly recommended the use of the alkalies. I therefore took the lixivium, and two bottles of PERRY's solvent; but the red deposit in my urine continued, my loins felt weak, and when in bed very painful.

Being in the profession of the law, and much employed, I was under the necessity of leading a very sedentary life, which so aggravated my tendency to bile and indigestion, that I seldom could get above two or three hours sleep.

With a view to alleviate these symptoms, and not with any idea of its being beneficial to the stone, I resorted to magnesia, which I continued with little intermission for eight months in the dose of a tea-spoonful or two, every evening before I went to bed. The long vacation coming on, I gradually took more exercise, and used the cold bath. The tone of my stomach, at the end of the period I have mentioned, was so far restored as to induce me to set medicine of all kinds aside, except when any food or drink disagrees, when I occasionally resort to the magnesia. Under such treatment, the weakness and pain in my kidney left me, and the red sand entirely disappeared. I

have since enjoyed a very good state of health, and am now in my fifty-seventh year.

If I occasionally make a little free with the good things of this world, my stomach reminds me of the improper use of the lixivium, especially when I am prevented taking my usual exercise.

The above case is important, not only as furnishing a striking and unprejudiced instance of the effect of magnesia, in counteracting the tendency to form uric calculi and gravel; but likewise, as demonstrating its efficacy where the alkalies had failed, and where the digestive organs had been injured in consequence of the use of such remedies: the time which has elapsed since the cure of this and other cases, without a relapse, is also strongly in favour of this mode of treatment.

Case 2. A gentleman twenty years of age who had suffered from heartburn, and other dyspeptic symptoms, was seized, on the 1st of June, 1811, with a violent pain in the loins, and more especially in the right kidney, and during the night he passed a large quantity of red sand with his urine. On the 2d, with a view to relieve the pain, which had increased considerably, he took fifty drops of laudanum, and drank freely of barley water. The night was passed more quietly, but on the morning of the 3d, he was seized with a violent pain in the kidney, and with the usual symptoms of the passage of a calculus along the ureter. These continued with more or less violence till the evening of the 4th, when he became perfectly easy, and remained so till the morning of the 6th, when, with

considerable pain and difficulty, he voided a calculus composed of uric acid, weighing nine grains. For several successive days his urine deposited a large quantity of red sand, and three very small round calculi were voided.

He was now directed to abstain from all kinds of fermented liquors and sour food, and to take a pint of treble soda water, (containing three drachms of sub-carbonate of soda,) daily. Under this treatment he continued to recover, and remained perfectly free from complaint until the end of August, when a copious deposit of red sand appeared in his urine: he had little pain in the affected kidney, but complained of almost constant nausea, or want of appetite. The soda water was increased to a pint and a half, and afterwards to two pints daily, and in the intervals he drank very freely of barley water.

Having persevered in this way for ten days without receiving any benefit, he was induced to make a trial of magnesia, of which he took one tea-spoonful night and morning in cold chamomile tea. In about a week, the state of his stomach was much improved, and the deposit in the urine proportionally diminished, and in three weeks every symptom of disease had disappeared.

In February, 1812, having persevered in the use of magnesia with little intermission, I was informed that the sand had returned, that increasing the quantity of magnesia had produced no good effect, and that alkalies materially aggravated his complaint, by disagreeing with the stomach and greatly increasing the urinary deposit.

On examining the sand, I found that instead of consisting as formerly of uric acid, it was composed of a mixture of the

ammoniaco-magnesian phosphate with phosphate of lime; he was directed to abstain from magnesia and alkalies, and to adopt a plan of treatment which it is the object of the second section of this paper more particularly to explain.

The foregoing is a well marked case of uric gravel with a strong tendency to form calculi, materially relieved by the use of alkaline remedies: it illustrates their usual effects when carelessly persevered in, and shews the advantage with which magnesia may in such instances be employed: it also exhibits the effect of magnesia and the alkalies, in producing the deposit of *white sand* (or phosphates) in the urine, when the *red sand* (or uric acid) has been removed.

The cases which follow are selected, from among others, to explain the best mode of preventing the formation of white sand, and to shew the most effectual treatment where it is a natural deposit in the urine, or where it has been induced by the incautious exhibition of alkaline medicines.

SECTION II.

The white sand so frequently voided by persons labouring under calculous complaints, was first analyzed by Dr. WOLLASTON,* who found it composed of ammoniaco-magnesian phosphate, either alone or mixed with variable proportions of phosphate of lime. The use of acid medicines in these cases was also first suggested by the same able chemist, but although his valuable observations have been before the public for nearly fifteen years, I am not aware that any

* Phil. Trans. 1797.

experiments have been made to ascertain what acids are best calculated to produce the desired effect, or to illustrate their mode of action.

Since my former communication, I have lost no opportunity of attending to this important subject, and hope that the conclusions, suggested by the following cases, will be deemed satisfactory, and that their application in practice may lead to useful results.

Case 1. A gentleman, fifty years of age, who about ten years before had undergone the operation for the stone,* was attacked on the 14th of January, 1810, with violent pain in the right kidney and ureter, which lasted two days; on the 17th, these symptoms subsided, and were followed by those of stone in the bladder, which continued for some days, and although he had taken abundance of barley water and similar diluents, the stone shewed no disposition to pass. On account of his former sufferings, this circumstance rendered him extremely uneasy, and on the evening of the 21st, he suffered several severe paroxysms of pain on attempting to make water. Under these circumstances, he was desired to take a purge, composed of two ounces of infusion of senna, two drachms of tincture of senna, and twenty grains of powdered jalap.† In three hours this began to take powerful effect, and during the

* The stone extracted consisted of a nucleus of uric acid about the size of a pea, incrusted with a mixture of the phosphates. It was broken during the operation, but appeared to have been of the size of a pigeon's egg.

† I recommended this treatment in consequence of having heard Sir Everard Home state a case, in his Surgical Lectures, of a gentleman who suffered a bougie to pass so far into the urethra, that it could not be removed by any instrument. During the operation of a purge it was expelled with considerable force.

violence of the operation, he was so fortunate as to void the calculus with his urine; it weighed eight grains. On the 28th he again suffered pain in the region of the kidneys, and voided much sand, composed of uric acid, with ammoniaco-magnesian phosphate. He now took three half pints of soda water daily, which materially increased the proportion of the triple phosphate, while that of uric acid was considerably diminished. Ten drops of muriatic acid were then taken three times a day in water. The red sand now began to re-appear, and on the 4th of February, he voided a very small uric calculus. The urine made after dinner contained more or less mucus streaked with blood, a symptom which was much aggravated by a slight excess in wine. On the 6th, he left London, and employed no medicine until the 12th, when he returned in consequence of having voided a large quantity of the white sand.

Having observed the efficacy of carbonic acid in preventing the deposition of the phosphates, and having found it less liable than any other acid to induce a return of the uric gravel and calculi, I now directed him to take half a pint of water highly impregnated with fixed air, four or five times a day, and to drink cyder instead of wine. On the 18th of February, his urine was less turbid than it had been for some months before, and on the 20th of March, having continued the use of carbonic acid, he had no remaining symptoms.*

* I have several times examined the urine, with a view to ascertain whether any of the acids which were exhibited, could be detected in that secretion; but the results of such experiments are so much interfered with by the very compound nature of the urine, that I have not hitherto been able to draw any satisfactory conclusions respecting them.

In August his urine became again turbid, but by the use of vinegar and lemon juice at his meals, which acids, he now finds, have no tendency to induce a return of the red gravel, he succeeds in preventing this symptom.

Case 2. On the 11th of October, 1812, the operation for stone in the bladder was performed upon a boy, eleven years of age, and four calculi were extracted, of which the largest was of the size of a small horse bean: they were each composed of a nucleus or centre of uric acid, upon which the ammoniaco-magnesian phosphate was deposited.

After the operation, the urine deposited a large quantity of white sediment, and some small pieces of red gravel were occasionally voided. He was now directed to take eight grains of citric acid dissolved in barley water, three times daily; under this treatment the sediment in the urine was considerably diminished, but did not wholly disappear. The dose of the acid was gradually increased to twenty grains, by which means the sediment was only occasionally deposited, and consisted of little else than mucus. It was observed, that whenever the citric acid was omitted, even for twenty-four hours, the sediment was greatly increased, and this was constantly attended with frequent desire to make water, and other symptoms of irritation in the bladder. On resuming the use of the citric acid, the sediment always disappeared, and the irritation of the bladder subsided, and this happened so frequently, that no doubt could be entertained of the influence of the medicine on the composition of the urine.

This plan of treatment was continued for three months; at the end of that period, it was found that the urine had not the

same disposition to deposit the phosphates as formerly; even when the medicine was omitted, the sediment was small in quantity, and not constant in its appearance. He was now directed to omit the use of the citric acid, and occasionally to eat oranges and other acid fruits. He continued this plan until the beginning of April, 1813; his urine was then quite clear, and he had no symptoms of disease.

Case 3. In the month of October, 1811, a gentleman, thirty-four years of age, informed me, that he had observed a white deposit in his urine, during the whole of the preceding summer. He had taken considerable quantities of soda water, which he thought increased the sediment, and alkalies in any other form produced a very obvious aggravation of the complaint.

His urine was at all times clear when voided; but after a few hours, a white powder was observed to separate from it, and a film of crystalline matter formed upon the surface. The former consisted of phosphate of lime and mucus, the latter of the ammoniaco-magnesian phosphate.

He was directed to take one drachm of muriatic acid properly diluted, at divided doses, during the day; and it was proposed that he should pursue this plan for a week; but it was discontinued on the third day on account of its acting upon the bowels, and producing a frequent desire to make water.*

On the 10th of October, he was advised to take two large glasses of lemonade daily, and to substitute claret for port wine, a pint of which he was in the habit of drinking daily.

* In this and other instances the sulphuric and nitric acids were occasionally substituted for the muriatic; but they were found equally inadmissible.

Under this treatment the symptoms produced by the muriatic acid subsided; but the appearance of the urine was not at first improved.

On the 20th, the film of triple phosphate formerly constantly observed in the urine began to decrease, but the white sand remained as abundant as before; he was therefore directed to take twenty grains of citric acid twice a day, and to continue the use of acid drink, as formerly.

The additional acid at first disagreed with the bowels; but this effect soon ceased, and the sediment was only observed in the urine voided in the morning; he therefore took another dose of the acid every night. This plan was pursued with little intermission until the beginning of December: the deposition of the phosphates gradually ceased, and he remained in perfect health until the middle of May, 1812, when after violent exercise and taking more wine than usual, the white sand again made its appearance in great abundance; his stomach became extremely irritable, and the acids, which he had before employed with success, brought on considerable irritation in the bladder. The addition of ten drops of laudanum to each dose of the citric acid prevented this effect, and he was thus enabled to continue the acid, which in a fortnight relieved his complaint.

This gentleman informed me, that whenever he omitted the use of an acid diet, or took much wine, especially port, his urine deposited the white sand and mucus, for two or three successive days.

Case 4. A gentleman, eighty years of age, who had twice submitted to the operation for the stone within five years,

4. That vegetable acids, especially the citric and tartaric, are less liable to produce the last mentioned effects, even when taken in large doses for a long time; and that carbonic acid is particularly useful in cases, where the irritable state of the bladder prevents the exhibition of other remedies.

XXVII. *Additions to an Account of the Anatomy of the Squalus Maximus, contained in a former Paper; with Observations on the Structure of the Branchial Artery. By Sir* Everard Home, *Bart. F. R. S.*

Read June 24, 1813.

My former account was taken from a Squalus Maximus caught at Hastings, in November, 1808, and the parts which I examined were brought to London by Mr. Clift, who went down, at my desire, to dissect them; but the weather being stormy and cold, the fish was brought no further than the beach, so that the examination was conducted under great disadvantages, and the parts brought away were in a mutilated state. The sketch of the fish made upon the spot by Mr. Clift is now found to be generally correct, except the omission of a small fin between the anus and tail, which had been buried by the weight of the fish in the sand.*

Two fishes of the same species have since been caught at Brighton, and one of them was brought to London in December, 1812, which I had an opportunity of examining, assisted by Mr. Clift. It is not my intention, on the present occasion,

* The omission of this small fin in the drawing is an error of considerable importance, as it deprived the fish of one of its characteristic marks, and has led naturalists, who have since had the opportunity of examining other specimens with more accuracy, to conclude that this fish was a distinct species from those which they described; I am therefore particularly desirous to correct the mistake.

to enter into minutiæ, but to render my former account more complete, and apply the dissection of this large fish to its proper use in comparative anatomy, which is, by means of it, to illustrate the functions of the organs of fishes of an ordinary size.

In addition to my former description of the fins, I have now annexed a drawing (Pl. XVI.) which shews the structure of the pectoral fin, a beautiful example of the mechanism of the fins of cartilaginous fishes in general.

The stomach was examined in its entire state, and the annexed drawing (Pl. XVII.) is an accurate representation of the appearance of its internal surface, which is exposed in one view, shewing that the pyloric portion is longer and narrower than I had before represented it.

The situation of the pancreas is correctly noticed in my former paper; the gland is oblong, thick and round where it is attached to the duodenum, and becoming thin, flat, and bifid towards its loose extremity.

The ducts of the liver are six in number, and inclosed in a broad flat band, which passes obliquely down before the stomach, till it is connected to the duodenum; each of the ducts opens, by a separate oblique orifice, into a common cavity of an oval form, from which there is a direct opening into the duodenum. This swell or enlargement might be considered as a substitute for the gall-bladder, which is wanting, were it not that a similar enlargement is also met with in fishes which have one. In the cod there is the same dilatation, and the hepatic ducts open into it in the same oblique manner; but there is also a gall-bladder, and the cystic duct, as well as the others, terminates in this dilatation.

The oblique openings of the hepatic ducts in the Squalus

Maximus, being so different from those of quadrupeds, explain the general principle on which the hepatic ducts in fishes, whose livers are loaded with oil, are formed. The substance of their liver is so exceedingly tender, that this contrivance is employed to prevent the bile from being forced back into the liver, which is not found necessary in the solid livers of land animals.

The heart was particularly examined, and the annexed drawing (Pl. XVIII.) shews its internal cavities, and the valves of the branchial artery; more particularly a muscular structure met with in the coats of that vessel, extending for some way after it leaves the ventricle.

The situation of the kidneys is mentioned in my former account; the ureters open into the cavity, common to the urine and semen, by two orifices, three quarters of an inch in diameter.

The structure of the body of the testicles had been destroyed; the epididymis consisted of innumerable convolutions of a tube three-eighths of an inch in diameter, at the upper part coiled up into two or three lobes or masses, which could not be unravelled, from which the vas deferens went off, making irregular convolutions down towards the anus. The lower part for three feet in length is straight, and the canal seven inches in diameter, having broad valvulæ conniventes; the widest are near the termination, and are two inches and a half broad. The contents of this portion, as I have remarked in my former paper, are different from those of the epididymis, and upper portion of the vas deferens, being a substance like starch broken down into rounded portions in a thinner fluid.

This circumstance leads to the idea, that the straight portion

of the vas deferens is analogous to the vesiculæ seminales of other animals, secreting its own fluid with which the semen is to be mixed before it enters the penis, and the valvulæ conniventes form a surface from which this secretion probably takes place.

It is deserving of observation, that the epididymis and vas deferens were loaded with semen, which there is reason to believe is not the case in quadrupeds, unless immediately previous to the act of copulation; but in fishes, where the connexion between the sexes is less complete, the semen appears to be prepared at a more early period.

The openings from the vasa deferentia are situated on each side, four inches higher up than those of the ureters, rounded in form, and kept closed by the pressure of two oviform ligamentous substances; these openings readily admit of dilatation, so that the hand can pass into them.

The penis has an infundibular form, and terminates by an oblique aperture about three inches in circumference.

The holders correspond with those of the dog-fish, which upon another occasion have been described to the Society; the spur bears a striking resemblance to that of the male ornithorhynchus paradoxus. There is a canal in each holder communicating with a corresponding cavity between the skin and muscles of the abdomen, eleven feet long and two wide. The inner surface of this cavity is smooth, almost polished, and of a beautiful white colour; it contained a white mucus, extremely viscid and tenacious.

Opportunities of examining the brain of a fish of such magnitude are of rare occurrence; I have therefore not only given drawings of the brain, (Pl. XIX, XX.) but also one

of the brain of a Squalus Acanthias, (Pl. XXI.) that the two may be compared together, with a view to shew the relative size of the parts, one belonging to a shark of thirty feet, the other to one of three feet long.

In the brains of what we consider the animals of greatest intelligence, there is a cerebrum, cerebellum, and medulla oblongata; beneath the cerebrum there are the tubercula quadrigemina. In fishes, the cerebrum is wanting, and there is no part at all analogous to it, unless we consider the enlargements, from which the olfactory nerves arise, to be of that description.

These enlargements are separated from the other parts of the brain by the optic nerves going off in a transverse line between them and the tubercula quadrigemina. In the present specimen, unfortunately, not only the enlargements from which the olfactory nerves go off, were destroyed; but also a portion of each of the anterior tubercula quadrigemina: the cerebellum was, however, entire, and is represented of the natural size.

The brain does not occupy more than one-third of the cranium. The medulla spinalis is large in proportion to the brain. From the termination of what corresponds to the calamus scriptorius in the human brain, a fissure extends on the upper part of the medulla spinalis, dividing it longitudinally into two portions; there is a similar fissure on the anterior surface; into both of these a thin fold of pia mater extends and adheres with firmness to the surfaces with which it comes in contact.

The dura mater is very dense, and adheres firmly to the inner surface of the cranium and theca vertebralis. The pia

mater nearly resembles that of the human brain; it becomes thicker where it covers the spinal marrow. The space between the dura and pia mater is occupied by a cellular membrane of a very fine texture.

As the different parts of the brain are described in the explanation of the drawing, I shall only remark in this place, that the circumstance most deserving of observation respecting it is, that the cerebellum has an increase of size in the Squalus Maximus in a much greater degree beyond that of the Squalus Acanthias than the tubercula quadrigemina. The protuberances from which the olfactory nerves arise were probably large in the same proportion with the tubercula; at least, in the brain of a shark, preserved in the Hunterian Collection, of a smaller size than that of the Squalus Maximus, but much larger than that of the Squalus Acanthias, that is the case.

The eye is small for the size of the fish; the ball has projections on the sclerotic coat, where the muscles are attached, which make it approach to a quadrangular form; but its internal cavity is circular. The circumference in the widest part is nine inches. The longest diameter three inches, the shortest one inch and three quarters. The sclerotic coat is cartilaginous, one quarter of an inch thick on the posterior part, becoming thinner towards the ciliary processes, where it is only one-sixteenth of an inch.

The cornea is thin, but made up distinctly of three layers, of which the middle one is by much the thinnest. The optic nerve is nearly of the size of the sixth pair, and, where it perforates the sclerotic coat, projects a little before it gives off the retina, which is extremely thin.

The choroid coat is covered with a tapetum lucidum of the

colour of an amalgam of silver broken into small portions. There are ciliary processes, not common to fishes in general; they are about one-third of an inch in extent, and slightly projecting; they are lined with a black pigment.

The vitreous humour is unattached to the choroid coat; it is inclosed in strong cells, and the crystalline lens, which is spherical and one inch in diameter, is imbedded in it for two-thirds of its substance. In the Tetrodon Mola, which, as well as the Squalus Maximus, in common language has been called the Sun-fish, the vitreous humour has a firm attachment to a groove in the choroid coat one-twelfth of an inch in breadth, extending from the entrance of the optic nerve to the termination of the retina, in the shortest line from the one to the other, and there are no ciliary processes; two such remarkable differences in eyes of nearly the same size, appeared to be deserving of being noticed.

The cartilage upon which the ball of the eye rests is attached to the bottom of the orbit, and is seven inches and a half long; its stem is a flattened cylinder, five-eighths of an inch in diameter; it terminates in a broad concave surface in a transverse direction adapted to the bottom of the ball of the eye, and is connected to the sclerotic coat by a ligament long enough to admit of motion.

There are four straight and two oblique muscles; the straight very large, much beyond what can be required merely to move so small an eye: the rectus internus and externus are strongest, they are five inches in circumference, while the superior and inferior are only three and a half.

In the structure of the ear, the only remarkable circumstance is the great capacity of the cavities in which the semi-

circular canals are contained, the canals themselves not being much larger than in the skate. They are shewn in the drawing. In the recent subject they were pellucid, containing a transparent fluid.

On the Structure of the Branchial Artery.

The muscular structure of the branchial artery of the dog-fish, and the direction in which that artery leaves the ventricle, are exactly the same as in the Squalus Maximus, only are seen upon so small a scale, that they do not arrest our attention; but when magnified, to the size which they acquire in this fish, they make a stronger impression on the mind, and force us irresistibly to enquire into their use.

This direction of the artery appears to be common to fishes in general; but the muscular structure is confined to particular tribes. I find it is common to all the sharks, and there is a similar structure in the sturgeon.

In the wolf fish, the Anarchichus Lupus, the muscular structure of the branchial artery is nearly the same; but the valves are placed close to the opening of the ventricle, and are only two in number.

In the turbot there is no muscular structure in this part of the artery; but the coats are extremely elastic, and admit of being very considerably dilated, particularly at its origin, where three valves are placed, and so contrived that the dilatation of the artery makes them shut more closely.

In the Lophius Piscatorius, there is no appearance of muscularity in the coats of the branchial artery, and no lateral valves as in other fishes; but there is a muscular tube half

an inch long, rising from the edge of the opening of the ventricle which projects into the artery.*

These different structures, so very unlike one another, and bearing no resemblance to the mechanism of the same parts in quadrupeds, make it probable, that the circulation through the gills is impeded by the external pressure of the water, in different degrees according to the depth of the fish from the surface: therefore in those fishes which frequent great depths, as the Squalus Maximus and all the shark tribe, there is a muscular structure in the coats of the branchial artery, which, when the fish is deep in the water, by its contraction diminishes the area of the vessel, and makes the valves perform their office; but when the fish is near the surface, this muscular structure, by its relaxation, renders the area of the artery so wide, that regurgitation of the blood takes place into the ventricle, and prevents the small vessels of the gills from being too much loaded.†

In fishes that swim deep, and do not come to the surface, as the wolf fish, the regurgitation does not take place into the ventricle; but the relaxation of this muscular portion of the artery allows it to dilate, and form a reservoir, and the valves remain closed, so as to prevent more blood leaving the ventricle. In fishes residing at moderate depths, as the turbot, elasticity is employed as a substitute for muscular power, there being less variation in the pressure made upon the gills; but in the Lophius Piscatorius, which probably never descends into

* This structure, which it is difficult to describe, will be better understood by a reference to the annexed drawing. (Pl. XXII.)

† That such regurgitation takes place when the muscle is relaxed, is ascertained by the ventricle being readily injected after death with common wax injection from the artery, the valves allowing it to pass.

water of much depth, the ventricle is so weak, that the supply of blood to the gills is regulated by the contraction and relaxation of a muscular valve.

As water, according to the degree of pressure upon it, is capable of containing a greater quantity of atmospherical air, than under ordinary circumstances, such a supersaturated state of the water might compensate, with respect to the respiration of fishes, for the difficulties, which occur at great depths, of forcing the blood through the vessels of the gills: I enquired what evidence could be produced, of the water at great depths containing a more than ordinary quantity of air; my philosophical friends, to whom I proposed this question, said, that it was a point that had not been considered. I therefore resolved to put it to the test of experiment, and as I knew there was a well at Mr. COUTTS's in the Strand, which more than twenty-five years ago had been sunk four hundred feet below the surface of the water in the Thames, I requested permission to make the experiment in that well. A cylindrical vessel, with a valve above and below, was let down to the depth of one hundred and eighty-six feet, which is now the depth of the well; it came up full of clear water, which Mr. W. BRANDE ascertained to contain no greater proportion of atmospherical air, than is met with in common river water. It therefore appears that the supply of air to fishes is nearly the same at whatever depth they are from the surface.

Besides the guards which have been mentioned, to prevent the circulation through the gills from being improperly carried on, in particular tribes of fishes, the ventricle itself is so formed, and is so situated with respect to the auricle, that the blood received is first impelled in a direction nearly at right

angles to that of the artery, and then another set of muscles is employed to force it into the artery.

This mechanism, common to most fishes, and so different from what is met with in land animals, where the blood is forced by the action of the left ventricle into the different parts of the body, appears to be intended to prevent too great a force being employed, at any one time, in impelling the blood through the gills.

Having traced in these different gradations the guards upon the circulation through the gills in fishes, I am led to extend my remarks to the hearts of a lower class of animals. In the Mollusca there is no reason for putting guards upon the action of the heart, as in fishes, because in them the blood first supplies the different parts of the body, and only in its return passes through the gills; but there is a regulation of another kind, by means of which the circulation is increased or diminished, according to the activity or torpor of the animal. In the teredines, where a boring engine, requiring a great muscular power to work it, is almost constantly employed, the heart consists of two auricles and two ventricles; the auricles strong, cylindrical, and having valves between them and the ventricles; the ventricles themselves very strong, so that in fact there is a heart, composed of two auricles and two ventricles, both acting at the same time, and the blood is hurried on by a double power to supply the muscles of the boring shells, and, in this part of its course, has a bright red colour.

In the Oyster, on the other hand, the heart consists of only one auricle and one ventricle, both very weak in their muscular power, when compared with those of the teredines, although belonging to an animal of larger dimensions; but the heart is

laterally connected to the great muscle that shuts the shell, so that whenever that muscle is actively employed, its lateral swell presses against the heart, and forces out the blood, and by this means gives activity to the circulation, at the only time in which an increased circulation is particularly required.

In the Muscle, the heart can hardly be said to be distinctly divided into auricle and ventricle; but to consist of an oval bag, through the middle of which the lower portion of the intestine passes. Two veins from the gills open into the heart, one on each side, these may be considered as the auricles.

The coats of the ventricle are very thin, so as to have little power of propelling the blood; but this weakness appears to be compensated by the effect of the contraction and relaxation of the intestine contained in their cavity. This circumstance renders it probable, that the circulation goes on with activity while the processes connected with digestion are employed; but when the intestines are empty, there being no supply of nourishment, the circulation is not only very languid, but may possibly be entirely stopped.

In the caterpillar, the blood cannot be said to circulate, but is carried from one end of the animal to the other, in a tube which may be either called heart or artery, by a species of peristaltic motion. This ebbing and flowing of the blood, if such a term can be admitted, is greatly increased by the pressure upon the different portions of this tube, produced by the action of muscles employed in the progressive motion of the animal, so that the supply of blood to the different parts of the body is proportioned to the demand, which arises out of the bodily exertions.

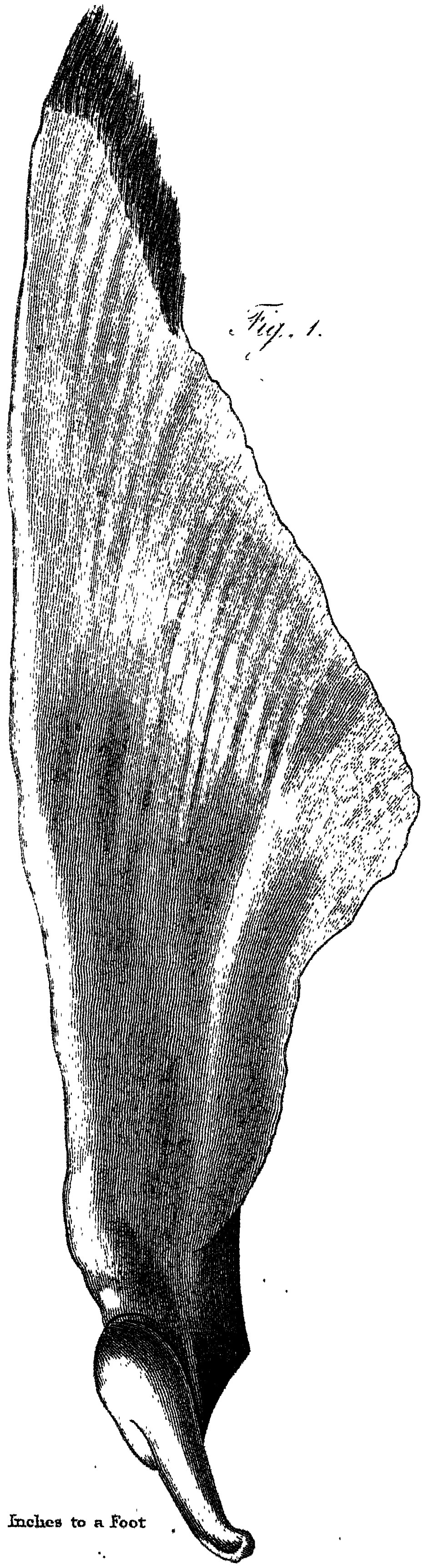


Fig. 1.

Two Inches to a Foot

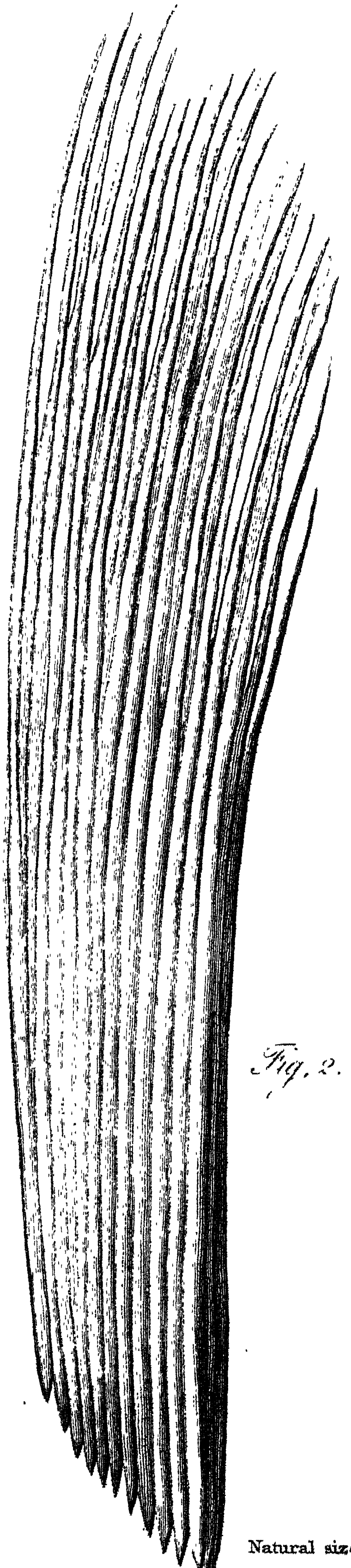


Fig. 2.

Natural size

One Inch to a Foot.

Fig. 1.

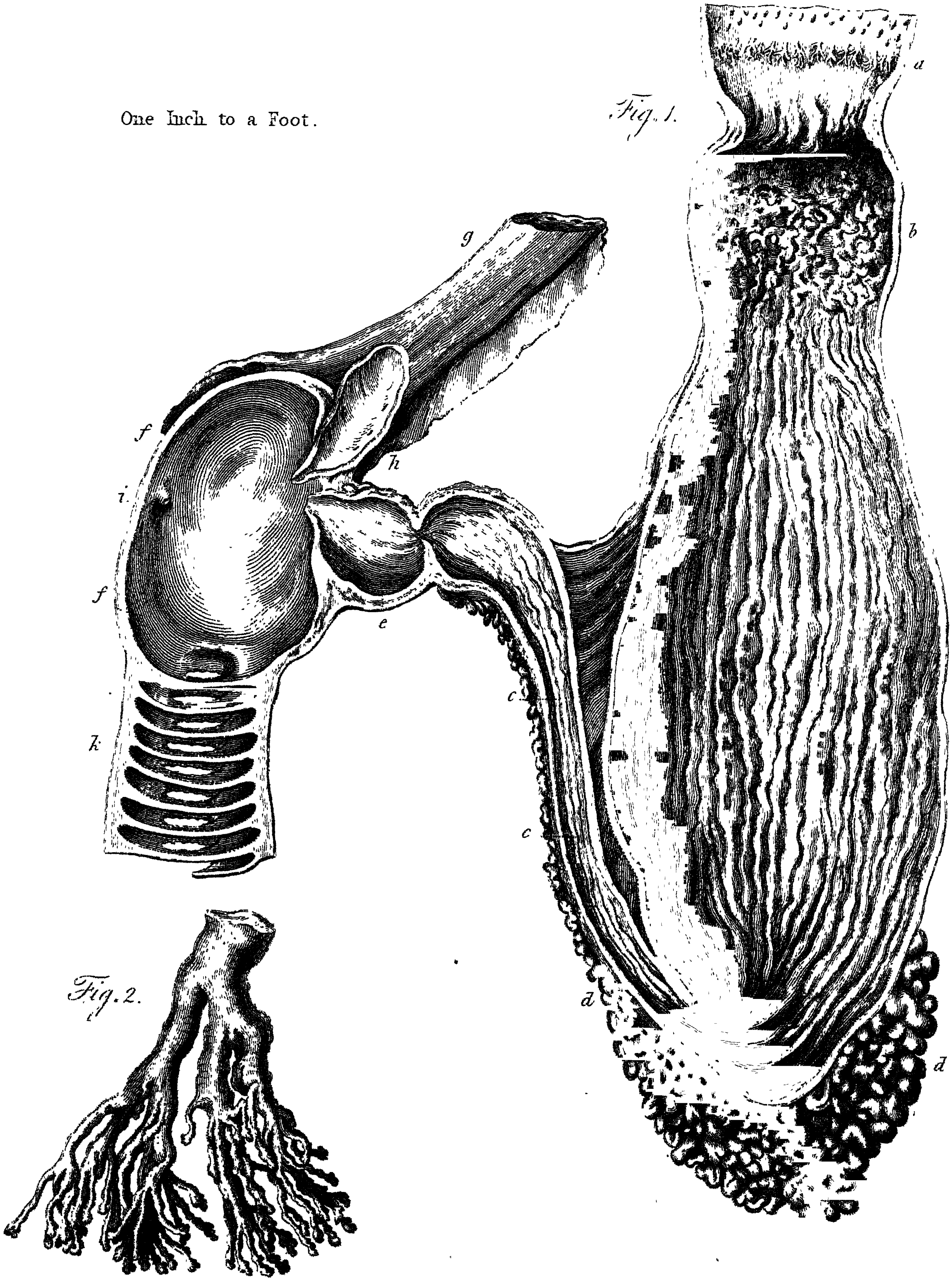
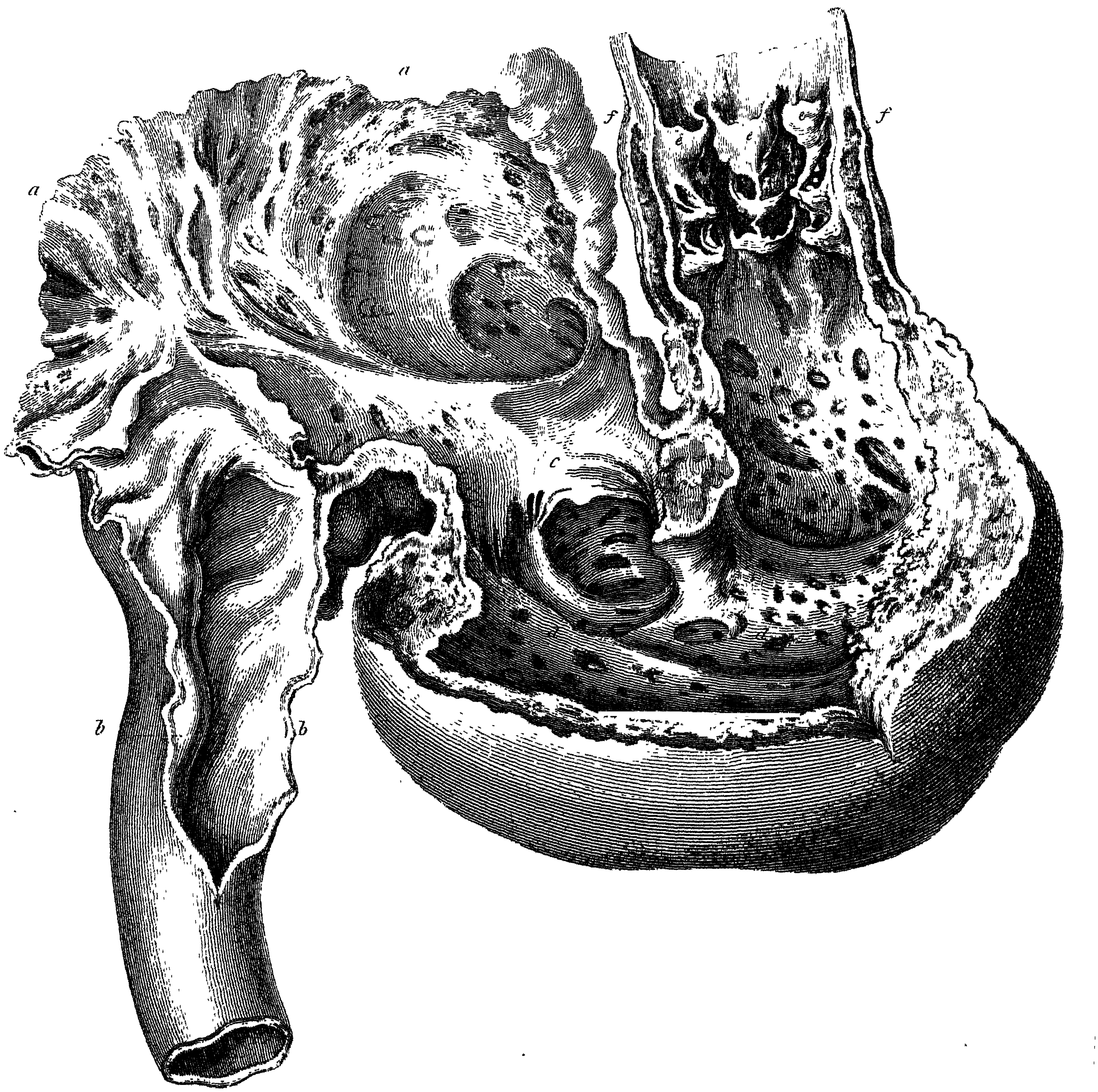


Fig. 2.

2 Inches to a Foot.



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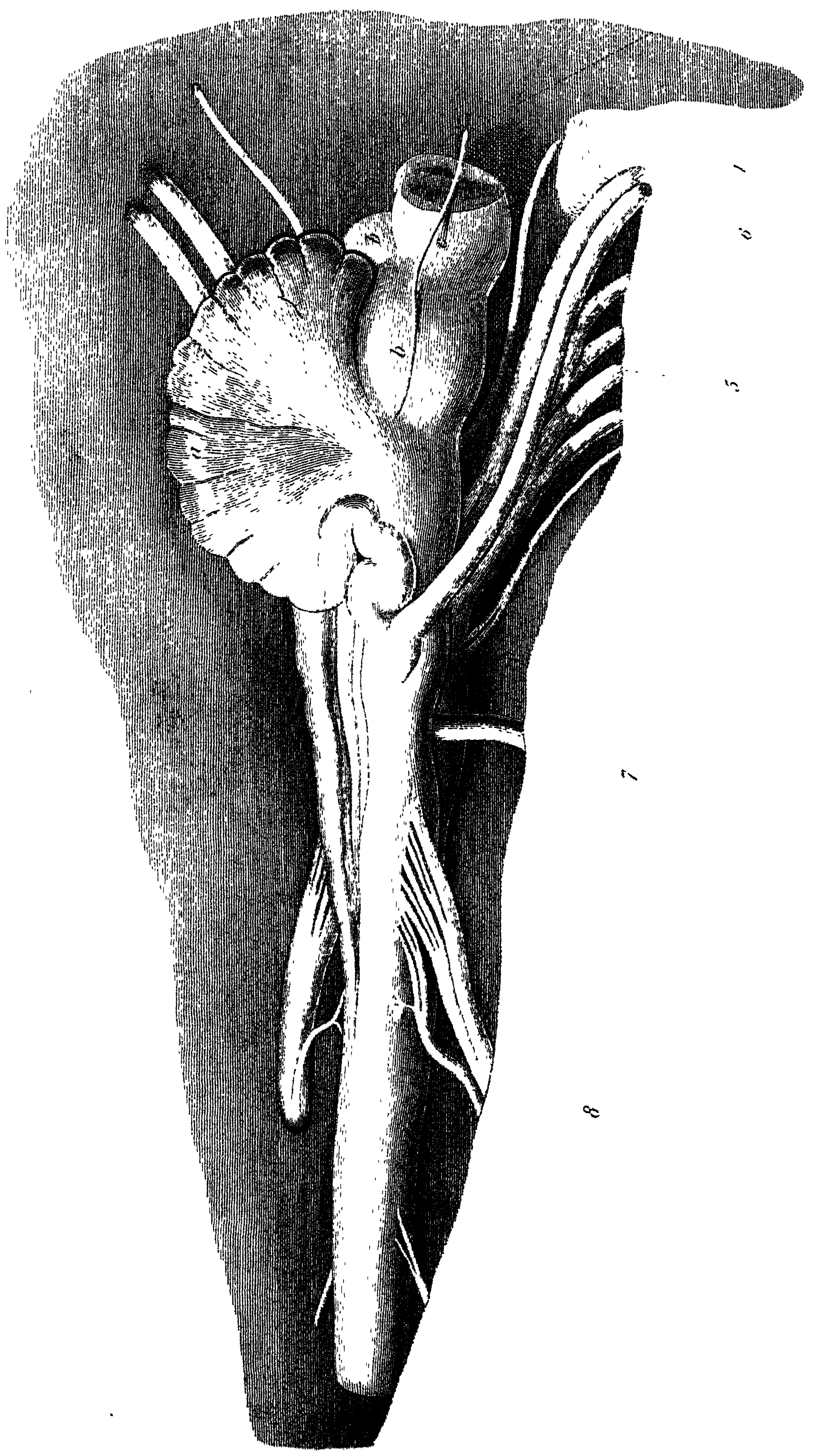

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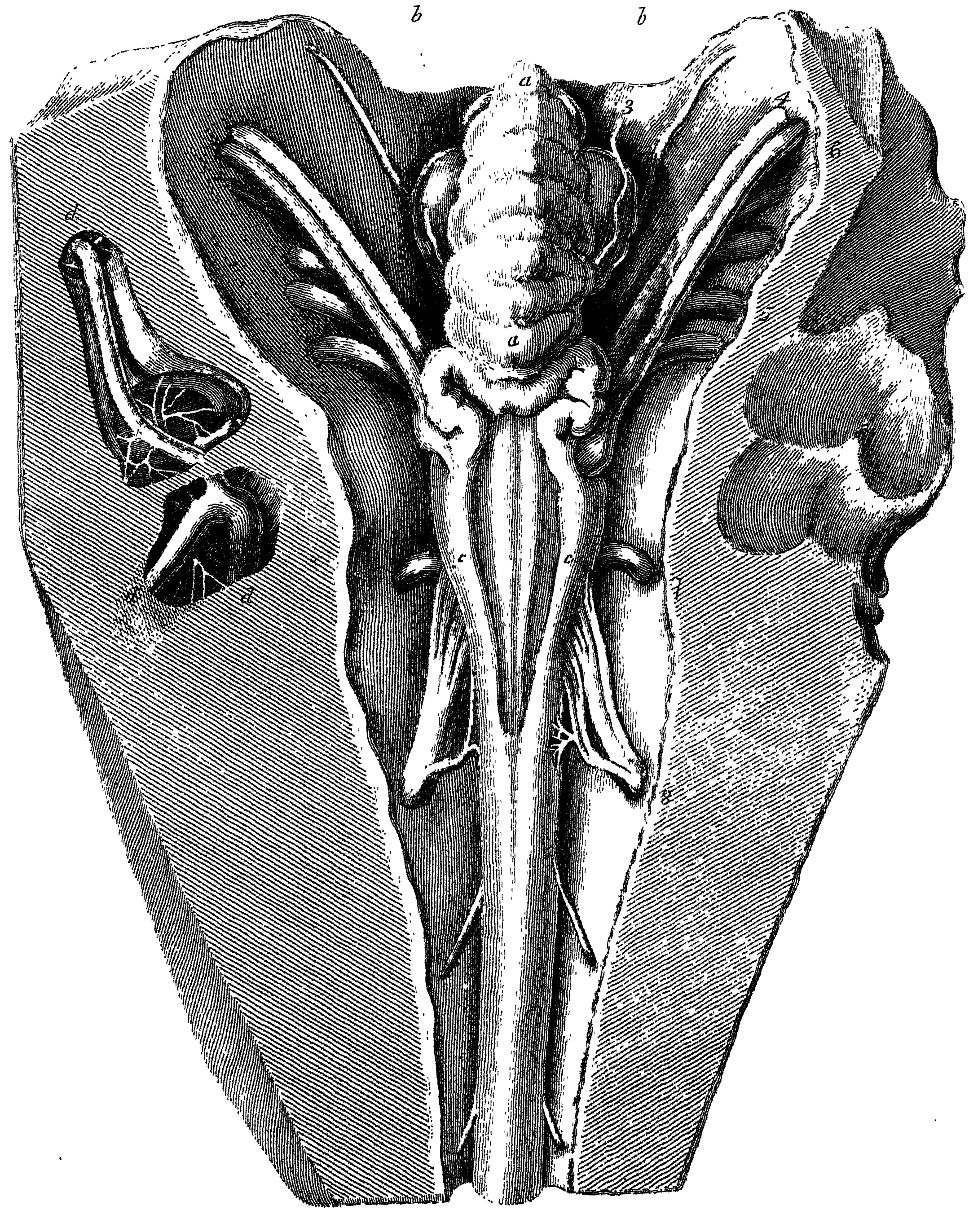

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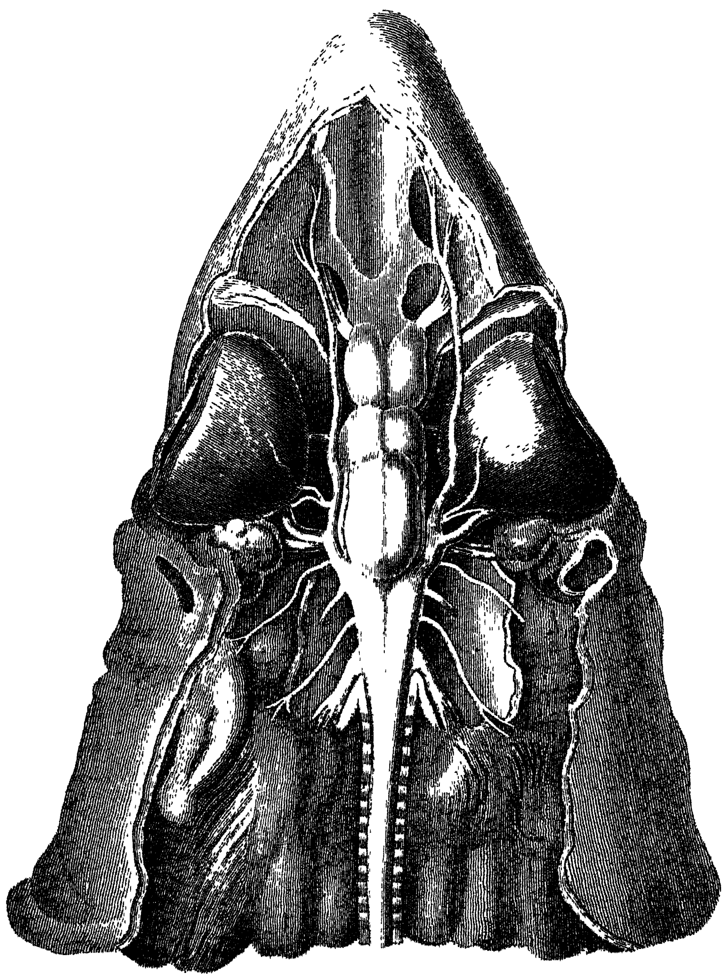

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EXPLANATION OF THE PLATES.

(See Plates XVI, XVII, XVIII, XIX, XX, XXI, XXII.)

Six of the following Plates are taken from drawings made by Mr. HOWSHIP, who very kindly undertook that task, during the indisposition of Mr. CLIFT, and his knowledge of anatomy, which he has cultivated with much ardour, induced me to solicit his assistance upon this occasion.

PLATE XVI.

Fig. 1. A view of the pectoral fin of the Squalus Maximus, from which the skin and cellular membrane are removed, to shew the arrangement of the cartilages of which it is composed.

Fig. 2. A portion of the elastic fibrous structure with which the fin is tipped, of its natural size.

PLATE XVII.

Fig. 1. An internal view of the stomach and duodenum of the Squalus Maximus.

a. The œsophagus.

bb. The cardiac portion of the stomach.

cc. The pyloric portion.

dd. The spleen.

e. A small cavity belonging to the stomach.

ff. The duodenum.

g. The band containing the hepatic ducts, six in number.

h. The dilatation in which the gall ducts terminate.

i. The opening of the pancreatic duct.

k. The spiral turns of the intestine.

Fig. 2. The fringe at the termination of the œsophagus of the natural size.

Plate XVIII.

The heart of the Squalus Maximus laid open.

a a. A portion of the internal surface of the auricle.

b b. One of the venæ cavæ laid open.

c. The valve between the auricle and ventricle.

d d. The cavity of the ventricle.

e e e. The three rows of valves and the three intermediate spaces, along which regurgitation takes place when the canal of the artery is dilated.

f f. The strong muscular covering of the artery.

Plate XIX.

A side view of the cerebellum, tubercula quadrigemina, and nerves of the brain of the Squalus Maximus.

3, 4, 5, 6, 7, 8. The different nerves going off from the brain corresponding with those in the brain of man.

a. Cerebellum.

b b. Two of the tubercula quadrigemina.

c. A third tuberculum mutilated.

Plate XX.

A view of the upper surface of the cerebellum and nerves of the Squalus Maximus.

The nerves going off marked, as in the last Plate, beyond which are two pair of nerves belonging to the spinal marrow.

a a. Cerebellum.

b b. Two of the tubercula quadrigemina.

c c. The part which corresponds with the fourth ventricle in the human brain, surrounded by an oval, continued, nervous band, from which the principal nerves go off.

d d d. Portions of the three semicircular canals of the ear in the cartilaginous cavity in which they are contained.

Plate XXI.

A view of the upper surface of the brain of the Squalus Acanthias, taken from a fish three feet long, to shew the difference of appearance and size between it and that of the Squalus Maximus.

The brain is entire, and the eyes are left in their situation, so that when this Plate is compared with that of the large brain, the parts that are wanting in it will be readily distinguished.

Plate XXII.

A view of the heart of the Lophius Piscatorius in a distended state, shewing the transparency of its coats, which are extremely thin.

a. The auricle.

b. The ventricle.

c. The branchial artery.

d. The projecting muscular tube serving as a valve.

XXVIII. *Some further Observations on a new detonating Substance. In a Letter from Sir* Humphry Davy, *LL.D. F.R.S. V.P.R.I. to the Right Hon. Sir* Joseph Banks, *Bart. K.B. P.R.S.*

Read July 1, 1813.

My dear Sir,

Berkeley-square, June 20, 1813.

I have already described, in a letter which you were so good as to communicate to the Royal Society, a few facts respecting a new detonating compound. I shall now do myself the honour of mentioning to you some other particulars on the subject.

I received, in April, a duplicate of the letter in which the discovery was announced, containing an Appendix, in which the method of preparing it was described. M. Ampere, my correspondent, states that the author obtained it by passing a mixture of azote and chlorine through aqueous solutions of sulphate, or muriate of ammonia. It is obvious, from this statement, that the substance discovered in France, is the same as that which occasioned my accident. The azote cannot be necessary; for the result is obtained by the exposure of pure chlorine to any common ammoniacal salt.

Since I recovered the use of my eyes, I have made many experiments on this compound; it is probable that most of them have been made before in France; but as no accounts of the investigations of M. Dulong on the substance have

appeared in any of the foreign journals which have reached this country, and as some difference of opinion and doubts exist respecting its composition, I conceive a few details on its properties and nature will not be entirely devoid of interest.

I have been able to determine its specific gravity, I hope, with tolerable precision, by comparing its weight at 61° FAHRENHEIT, with that of an equal volume of water. 8,6 grains of the compound, carefully freed from the saline solution in which it was produced, filled a space equal to that filled by 5,2 grains of water, consequently its specific gravity is 1,653.

When the compound is cooled artificially, either in water or in solution of nitrate of ammonia, the fluid surrounding it congeals at a temperature a little below 40° FAHRENHEIT, which seems to be owing to its becoming a solution of chlorine; for, as I have stated in a paper published in the Philosophical Transactions, the saturated solution of chlorine in water freezes very readily. The congelation of the fluid, in contact with the new compound, led me, when I first operated on it in very small quantities, to suppose it readily rendered solid by cooling; but I find in experimenting upon it, out of the contact of water, that it is not frozen by exposure to a mixture of ice and muriate of lime.

The compound gradually disappears in water, producing azote, and the water becomes acid, and has the taste and smell of a weak solution of nitro-muriatic acid.

The compound, when introduced into concentrated solution of muriatic acid, quickly resolves itself into gas, producing much more than its own weight of elastic fluid, which proves to be pure chlorine, and the solution evaporated affords muriate of ammonia.

In concentrated nitric acid it afforded azote.

In diluted sulphuric acid it yielded a mixture of azote and oxygen.

It detonated in strong solutions of ammonia. In weak solutions it produced azote.

It united to or dissolved in sulphurane, phosphorane, and alcohol of sulphur, without any violence of action, and dissolved in moderately strong solution of fluoric acid, giving it the power of acting upon silver.

When it was exposed to pure mercury, out of the contact of water, a white powder and azote were the results.

The first attempt that I made to determine the composition of the detonating substance, after my accident, was by raising it in vapour in exhausted vessels, and then decomposing it by heat; but in experiments of this kind, even though the whole of the substance was expanded into elastic matter, yet the vessel was often broken by the explosion, and in several instances violent detonations occurred during the process of exhaustion, probably from the contact of the vapour of the substance with the oil used in the pump.

In the only instance in which I was able to examine the products of the explosion of the substance in an exhausted vessel, no muriatic acid or water was formed, and chlorine and azote were produced; but it was impossible to form any correct opinion concerning the proportions of the gaseous matter evolved, as an unknown quantity of common air must have remained mixed with the vapour in the vessel.

The action of mercury on the compound appeared to offer a more correct and less dangerous mode of attempting its analysis; but on introducing two grains under a glass tube

filled with mercury and inverted, a violent detonation occurred, by which I was slightly wounded in the head and hands, and should have been severely wounded, had not my eyes and face been defended by a plate of glass attached to a proper cap, a precaution very necessary in all investigations of this body.

In using smaller quantities and recently distilled mercury, I obtained the results of the experiments, without any violence of action; and though it is probable that some accidental circumstance might have occasioned the explosion of the two grains, yet I thought it prudent, in my subsequent experiments, to employ quantities which, in case of detonation, would be insufficient to do any serious mischief.

In the most accurate experiment that I made, $\frac{7}{10}$ths of a grain of the compound produced, by its action upon mercury, 49 grain measures of azote. I collected the white powder which had been formed in this and other operations of the same kind, and exposed it to heat. It sublimed unaltered, without giving off any elastic or fluid matter, which there is the greatest reason to believe would not have happened, if the compound had contained hydrogen, or oxygen, or both. The sublimed substance had the properties of a mixture of corrosive sublimate and calomel.

If the results of this experiment be calculated upon, it must be concluded that the compound consists of 57 of azote to 643 of chlorine in weight, or 19 to 81 in volume; but this quantity of azote is probably less than the true proportion, as there must have been some loss from evaporation, during the time the compound was transferred, and it is possible that a minute quantity of it may have adhered to mercury not immediately within the tube.

The decomposition in this process is very simple, and must be supposed to depend merely upon the attraction of the mercury for chlorine, in consequence of which the azote is set free; and if the result does not strictly demonstrate the proportions of chlorine and azote in the compound, yet it seems at least to shew, that these are its only constituents.

As muriate of ammonia and chlorine are the only products resulting from its action upon solution of muriatic acid, it seems reasonable to infer, that this action depends on a decomposition of part of the muriatic acid, by the attraction of the new compound for hydrogen to form ammonia, which, at the moment of its production, combines with another portion of the acid, the chlorine of both compounds being set free.

On this view, the quantity of chlorine formed from a certain quantity of the compound being known, it becomes easy to determine the composition of the compound; for, ammonia being formed of three volumes of hydrogen and one of azote, and muriatic acid of one volume of hydrogen and one of chlorine, it is evident, that for every three volumes of chlorine evolved by the decomposition of muriatic acid, one volume of azote must be detached from the compound; and the weight of chlorine in the compound must be less than the weight of the whole quantity of chlorine produced by a portion, which is to the azote in the compound as 295 to 2295, if the relative specific gravities of the two gases be considered as 2,627 and 1.

Two grains of the compound, when exposed at the temperature of 62° FAHRENHEIT, and under a pressure of the atmosphere equal to that of 30,1 inches of mercury to strong solution of muriatic acid in a proper apparatus, afforded 3,91 cubic inches of chlorine.

In another experiment, one grain of the compound afforded 1,625 cubic inches of chlorine.

In a third experiment, one grain produced only 1,52 cubic inches.

In the two last experiments the compound was acted upon much more slowly, and the gas generated exposed to a much larger surface of solution of muriatic acid, and the appearance of a smaller relative proportion of chlorine must be ascribed to the absorption of a larger proportion of that gas by the liquid acid; and I found by exposing concentrated solution of muriatic acid to chlorine, that it soon absorbed nearly its volume of that gas.

I attempted to remove the source of error in the experiment, by using liquid muriatic acid holding chlorine in solution; but in this case the quickness of the action of the compound on the acid was greatly diminished, and it not being easy to obtain the point of absolute saturation of the acid with chlorine, some of the gas was absorbed in the nascent state during its slow production; and in most of my experiments made in this manner, I obtained less chlorine from a given weight of the compound, than in operating on pure solution of muriatic acid.

Liquid muriatic acid, whether concentrated or diluted in its pure state, does not affect the colour of the sulphuric solution of indigo; but it is immediately destroyed by solutions containing chlorine dissolved in them. The quantity of solution of indigo, which is deprived of colour by a given quantity of solution of chlorine, is directly as the proportion of chlorine it contains; and I found that the same quantity of chlorine, whether dissolved in a large or a small quantity of solution of

muriatic acid, destroyed the colour of the same quantity of the blue liquor.

On this circumstance it was easy to found a method of determining the precise quantity of chlorine produced in solution of muriatic acid, from a given quantity of the compound; namely, by comparing the power of a given quantity of muriatic acid, containing a known quantity of chlorine, to destroy the colour of solutions of indigo, with that of the muriatic acid, in which the compound had produced chlorine.

Two experiments were made. In the first, a grain of the compound was exposed on a large surface beneath a tube inverted in about six cubic inches of solution of muriatic acid, and the chlorine absorbed by agitation as it was formed. The acid so treated destroyed the colour of seven cubic inches of a diluted sulphuric solution of indigo; and it was found, by several comparative trials, that exactly the same effect was produced in another equal portion of the same solution of indigo, by 2,2 cubic inches of chlorine dissolved in the same quantity of muriatic acid.

In the second experiment, 1,3 cubic inches of chlorine were evolved in the gaseous form, the thermometer being at 58°, and barometer at 30,33, and suffered to pass into the atmosphere; and by the test of the solution of indigo, it was found that ,75 of a cubic inch remained dissolved in the acid.

Now, if the mean of these two experiments be taken, it appears that 1,61 grains of chlorine are produced in solution of muriatic acid by the action of a grain of the compound; and calculating on the data just now referred to, the compound must consist of 91 of chlorine and 9 of azote in weight, which

in volume will be nearly 119 to 30; and this estimation differs as little as might be expected from that gained by the action of mercury upon the compound.

It may fairly be concluded, that M. GAY LUSSAC's principle of the combination of gaseous bodies, in definite volumes, strictly applies to this compound, and that it really consists of four volumes of chlorine to one of azote; and the volumes likewise exactly coincide with the laws of definite proportions; and the detonating compound may be regarded as composed of one proportion of azote 26, and four proportions of chlorine 261.

I attempted a comparative experiment on the proportions in the compound, by estimating the quantity of azote produced in the decomposition of ammonia by it; but I found that this process was of no value for the purpose of analysis, for water appeared to be decomposed at the same time with the ammonia, and nitric acid formed; and, in consequence, the quantity of azote evolved was much less than it would have been, supposing the ammonia decomposed by the mere attraction of chlorine for hydrogen.

The results of the analysis of the new compound are interesting for several reasons.

They shew, what seemed probable from other facts, that there is no strict law of analogy, which regulates the combinations of the same substance with different substances. As three of hydrogen combine with one of azote, and one of hydrogen with one of chlorine, I thought it probable that the new compound would contain three of chlorine to one of azote, which is not the case.

This compound is the first instance known of one proportion

of a substance uniting to four proportions of another substance, without some intermediate compound of 1 and 1, 1 and 2, and 1 and 3; and the fact should render us cautious in adopting hypothetical views of the composition of bodies from the relations of the quantities in which they combine. Those who argue that there must be one proportion of oxygen in azote, because there ought to be six proportions in nitric acid, instead of five, which are produced from it by analysis, might with full as much propriety contend, that there must be azote in chlorine in some simple multiple of that existing in the compound.

It may be useful to shew, that many hypotheses may be framed upon the same principles; and which, consequently, must be equally uncertain. Views of this nature may be important in directing the practical chemist in his researches; but the philosopher should carefully avoid the developement of them with confidence, and the confounding them with practical results.

The compound of chlorine and azote agrees with the compounds of the same substance with sulphur, phosphorus, and the metals, in being a non-conductor of electricity; and these compounds are likewise decomposable by heat, though they require that of Voltaic electricity.

Sulphur combines only in one proportion with chlorine; and hence the action of *Sulphurane*, or Dr. THOMSON's muriatic liquor upon water, like that of the new compound, is not a simple phenomenon of double decomposition.

It seems proper to designate this new body by some name: *Azotane* is the term that would be applied to it, according to my ideas of its analogy to the other bodies which contain chlorine; but I am not desirous, in the present imperfect and

fluctuating state of chemical nomenclature, to press the adoption of any new word, particularly as applied to a substance not discovered by myself.

I am, my dear Sir,

very sincerely yours,

HUMPHRY DAVY.

XXIX. *Experiments on the Production of Cold by the Evaporation of the Sulphuret of Carbon. By* Alexander Marcet, *M.D. F.R.S. one of the Physicians to Guy's Hospital.*

Read July 8, 1813.

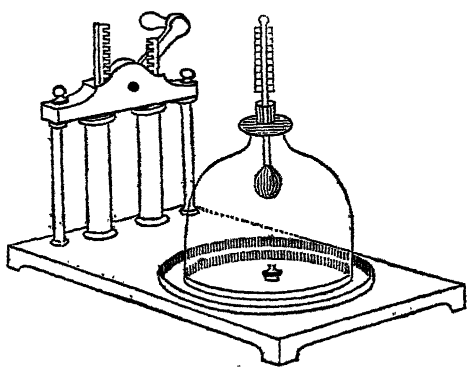
I HAD the honour, at an earlier period of this session, of giving to the Society, conjointly with Professor BERZELIUS, an account of the alcohol of sulphur, or sulphuret of carbon, and of noticing the remarkable volatility of that fluid. I have, since that period, tried a variety of experiments on the subject, and having found this compound more volatile than any other known body, and capable of producing, by its evaporation, a degree of cold of proportional intensity, I have been induced, in order to render the history of the sulphuret of carbon more complete, to present to the Society a brief account of my experiments.*

If the bulb of a small spirit thermometer be closely covered with a bag of fine flannel, or still better, with a piece of fine

* I would recommend to those, who may wish to repeat these experiments, to prepare this substance by means of a large earthen tube of about one inch and a half in diameter, instead of the small porcelain tubes, which are commonly used for this purpose. The process is always a tedious one; but a much more considerable quantity of the sulphureous liquor is procured by the larger tube in an equal space of time. The same tube can scarcely ever be used twice. About half a pint of the liquor may be obtained in one process; but the operation requires almost a whole day. Fresh pieces of sulphur may be successively introduced whilst the distillation is going on, and without renovating the charcoal, as the occasional admission of air does not materially affect the operation. The process would, no doubt, admit of further improvement.

lint, and a few drops of the sulphureous liquor poured upon it, the thermometer rapidly sinks from 60° to about 0; whilst if a similar experiment be made with ether, or alcohol, under the same circumstances, the temperature is reduced by the ether to only about + 20°, and by the alcohol to about 50°.* The hygrometrical state of the atmosphere appears to have some influence on these results.

If the bulb of the thermometer, thus wetted with the sulphureous liquor, be introduced into the receiver of an air-pump, (by means of a brass plate, as expressed in the annexed



sketch, the plate fitting the receiver air tight when laid upon its open neck), the thermometer sinks rapidly, as the ex-

* I have also tried some experiments on the comparative elastic force of the vapour of these fluids, by introducing in succession a few drops of them, through mercury, into a torricellian tube; and I obtained the following results:

	Sp. Grav.	*Temper.*	*Depression of Mercury.*	*Cold produced by Evaporation.*
Pure alcohol	0,806	66°	1,65	from 68° to 52°.
Sulphuret of carbon	1,272	66°	10,75	from 68° to 10°.
Rectified ether	0,724	66°	15,65	from 68° to 24.°

It would appear, therefore, that the degree of volatility of a fluid, or of cold produced by its evaporation, are far from bearing an exact proportion to the elastic force of its

haustion proceeds, and in one or two minutes it descends from + 70° to — 70°, or even 80° of FAHRENHEIT'S scale, that is at least 40 degrees below the freezing point of mercury. A cold of — 65 or — 70 is easily obtained by exhausting the receiver till the rarefied air supports only one quarter of an inch of mercury; but with a pump capable of exhausting to one-eighth of an inch, the thermometer sinks to — 81° or 82° in less than two minutes. It is scarcely necessary to observe that, if instead of a spirit thermometer, a tube containing mercury be employed, this metal may be instantly frozen, and the remarkable contraction, which it undergoes at the moment of its congelation, may be seen and estimated with great facility.*

These experiments are but slightly influenced either by the temperature of the atmosphere, or by that of the sulphureous liquor; but if the air be very damp, the moisture which accumulates upon the bulb whilst cooling, slightly impedes the process. This moisture appears in the form of a hoar frost, forming a snowy arborescent deposition all over the covering of the bulb. The oily liquor itself does not freeze when exposed to a temperature of at least — 60°.

I shall not take up the time of the Society by a detail of the various attempts which I have made to increase still farther

vapour. It may be observed also, that the elastic force of the vapour of alcohol of sulphur was stated by Mr. BERZELIUS and myself, in our joint paper, to be only 7,36 inches, instead of 10,75; but this difference is accounted for from the circumstance of the temperature, which in that instance was 53,5°, instead of 66°; a difference which is very nearly that which might have been expected from a comparison with the force of ether at the same temperature as given in Mr. DALTON'S tables.

* Mercury may also be frozen by a similar process, by means of ether, as I have stated in NICHOLSON'S Journal for February last.

the effects just related. The fact is, that all these attempts have hitherto failed, and the simplest mode of performing the experiment has uniformly yielded the best results. Indeed, I am now inclined to believe, that unless a substance were found which should exert an attraction upon the sulphureous liquor, without being itself evaporable, (as is the case with the sulphuric acid, in Mr. LESLIE's mode of freezing water), I have nearly reached the limit of cold that can be produced in this manner. It may be proper to add, that the presence of sulphuric acid in this experiment would have no immediate effect, as it exerts no attraction upon the volatilized liquor; yet by drying the rarefied atmosphere which surrounds the bulb, it might perhaps assist a little in promoting the process of evaporation. It may also be useful to caution those who may wish to repeat these experiments, against working the pump too rapidly; for in this case, a degree of heat is produced (in consequence, no doubt, of the condensed air rushing violently through the valve), which is sufficient to inflame the volatilized liquor mixed with it. This happens most frequently with pumps of large dimensions, and is immediately perceived by the sulphureous fumes which issue from the barrel; no other consequence ensues, however, except the injury done to the valves, which, at all events, are apt to be acted upon, and more or less deranged, by the repeated contact of the volatilized liquor. On this account the receiver used for these experiments should be of the capacity of two or three pints; for if it be very small, and the vapour very concentrated, the valves appear to be proportionally injured.

XXX. *On a saline Substance from Mount Vesuvius. By* James Smithson, *Esq. F. R. S.*

Read July 8, 1813.

It has very long appeared to me, that when the earth is considered with attention, innumerable circumstances are perceived, which cannot but lead to the belief, that it has once been in a state of general conflagration. The existence in the skies of planetary bodies, which seem to be actually burning, and the appearances of original fire discernible on our globe, I have conceived to be mutually corroborative of each other; and at the time when no answers could be given to the most essential objections to the hypothesis, the mass of facts in favour of it fully justified, I thought, the inference that our habitation is an extinct comet or sun.

The mighty difficulties which formerly assailed this opinion, great modern discoveries have dissipated. Acquainted now, that the bases of alkalies and earths are metals, eminently oxydable, we are no longer embarrassed either for the pabulum of the inflammation, or to account for the products of it.

In the primitive strata, we behold the result of the combustion. In them we see the oxyd collected on the surface of the calcining mass, first melted by the heat, then by its increase arresting farther combination, and extinguishing the fires which had generated it, and in fine become solid and crystallized over the metallic ball.

Every thing tells that a large body of combustible matter still remains enclosed within this stony envelope, and of which volcanic eruptions are partial and small accensions.

Under this point of view, an high interest attaches itself to volcanoes, and their ejections. They cease to be local phenomena; they become principal elements in the history of our globe; they connect its present with its former condition; and we have good grounds for supposing, that in their flames are to be read its future destinies.

In support of the igneous origin, here attributed to the primitive strata, I will observe, that not only no crystal imbedded in them, such as quartz, garnet, tourmaline, &c. has ever been seen enclosing drops of water; but that none of the materials of these strata contain water in any state.

a. The present saline substance was sent to me from Naples to Florence, where I was, in May 1794, with a request to ascertain its nature. The general examination which I then made of it, shewed it to be principally what was at that time called *vitriolated tartar*, and it was in consequence mentioned as such in an Italian publication soon after. But as this denomination, surprising at that period, was not supported by the relation of any experiments, or the citation of any authority, no attention was paid to it; and the existence of this species of salt, native in the earth, has not been admitted by mineralogists, no mention being made of it, I believe, in any mineralogical work published since.

b. I was informed by letter, that it had "flowed out liquid from a small aperture in the cone of Vesuvius," and which I apprehend to have happened in 1792 or 1793.

c. The masses of this salt are perfectly irregular, their texture compact, their colour a clouded mixture of white, of the green of copper, and of a rusty yellow, and in some places are specks and streaks of black.

d. A fragment melted on the charcoal at the blow-pipe formed hepar sulphuris.

e. A piece weighing 9,5 grains was so strongly heated in a platina crucible, that it melted and flowed level over the bottom of it, but did not lose the least weight.

f. Not the slightest fume could be perceived on holding a glass tube wetted with marine acid over some of this salt, while triturating in a mortar with liquid potash; but a similar mixture being made in a bottle, and which was immediately closed with a cork, to which was fixed a bit of reddened litmus paper, the blue colour of the paper was restored.

g. Being dissolved in water, there was a small sandy residue, which consisted of green particles of a cupreous nature, of a yellow ochraceous powder, and of minute crystals of a metallic aspect of red oxyd of iron, by which the black spots in the mass had been occasioned.* Mr. KLAPROTH found a similar admixture in muriate of soda from Vesuvius.†

h. The solution had a feeble green tint. It did not alter blue or reddened turnsol paper.

i. Prussiate of soda-and-iron threw down a small quantity of red prussiate of copper from it. Liver of sulphur and tincture of galls likewise caused very small precipitations.

* What mineralogists denominate speculary iron ore, *Fer oligiste* of Mr. HAÜY, appears to be merely red oxyd of iron in crystals; red hematite the same substance in the state of stalactite; and red ochres the same in a pulverulent form. The hematites which afford a yellow powder are hydrates of iron.

† Essays, Vol. II. p. 67, Eng. Trans.

j. Carbonate of soda, and oxalate of potash, and solutions of magnesia, clay, copper, iron, and zinc, either had no effects, or extremely slight ones.

k. Solution of sulphate of silver produced a white curd-like precipitate. 9,35 grains of this salt (the weight of the insoluble matter being deducted) afforded 1,05 grains of slightly melted muriate, or chloride, of silver. This precipitate was equally produced after the salt had been made strongly red hot, so that it was not owing to a portion of sal ammoniac.

l. Tartaric acid, and muriate of platinum, occasioned the precipitates in its solution which indicate potash.

m. Nitrate of lime did not form any immediate precipitate in a dilute solution of it; but in a short time, numerous minute prismatic crystals of hydrate of sulphate of lime were generated.

n. Nitrate of barytes poured into a solution containing 9,8 grains of this salt afforded a precipitate, which after being ignited weighed 12,3 grains. The filtered solution crystallized entirely into nitrate of potash mixed with a few rhomboides of nitrate of soda.

o. Some of this salt finely pulverized was treated with alcohol. This alcohol on exhaling left a number of minute cubic crystals, which proved, by the test of nitric acid, to be muriate of soda. Prussiate of soda-and-iron caused a red precipitate of prussiate of copper in this alcoholic solution.

p. The solution of this salt afforded, by crystallization, sulphate of potash in its usual forms, and some prismatic crystals of hydrate of sulphate of soda.

q. To discover what had occasioned the precipitate with galls, (*i*) since copper has not this quality, a portion of this

salt, which had been recovered by evaporation from a filtered solution of it, was made red hot in a platina crucible. On extraction of the saline part by water, a very small quantity of a black powder was obtained. Ammonia dissolved only part of it, which was copper. The rest being digested with muriatic acid, and prussiate of soda-and-iron added, a fine Prussian blue was formed.

r. From several of the foregoing experiments, it appeared that no sensible quantity of any of the mineral acids, besides the sulphuric and muriatic, existed in combination with alkali in this volcanic salt. But Mr. TENNANT, whose many and highly important discoveries have so greatly contributed to the progress of chemical science, having detected disengaged boracic acid amongst the volcanic productions of the Lipari islands, and suggested that it might be a more general product of volcanoes than had been suspected,* it became important to ascertain whether the presence of any in this salt proved Vesuvius likewise to be a source of this acid. Alcohol heated on a portion of it in fine powder, and then burned on it, did not however shew the least green hue in its flame.

s. To ascertain the proportions of the ingredients of this saline substance, the following experiments were made:

10 grains of sulphate of potash of the shops were dissolved in 200 grains of water, and an excess of muriate of platina added. The precipitate edulcorated with 100 grains of water, and dried on a water bath, weighed 24,1 grains.

10 grains of the saline part of the native salt, treated precisely in every respect in the same way, afforded 17,2 grains of precipitated muriate of platina-and-potash.

* Trans. of the Geolog. Soc.

If 24,1 grains of this precipitate correspond to 10 grains of sulphate of potash, 17,2 grains of it correspond to 7,14 grains of this salt.

It has been seen (*n*) that 10 grains of the saline part of this volcanic salt would have afforded 12,55 grains of sulphate of barytes.

But 7,14 grains of sulphate of potash form only 9,42 grains of sulphate of barytes,* and therefore the remaining 3,13 grains of sulphate of barytes would be produced by the sulphate of soda, and correspond to 1,86 grains of it in an arid state, or uncombined with ice.†

10 grains of the saline part of this native salt would have produced 1,12 grains of ignited muriate of silver (*k*). By accurate experiments 241 grains of ignited muriate of silver have been found to correspond to 100 grains of ignited muriate of soda.‡

Consequently the soluble portion of the present Vesuvian salt consists of

Sulphate of potash - -	7,14
Sulphate of soda - -	1,86
Muriate of soda - -	0,46
Muriate of ammonia, Muriate of copper, Muriate of iron - -	0,54
	10,00

t. The insoluble sandy residue (*g*) having been thoroughly

* Dr. MARCET on Dropsical Fluids.

† Prof. KLAPROTH's Essays, Vol. I. p. 282.

‡ Dr. HENRY, Phil. Trans. 1810.

edulcorated, dilute nitric acid was put to it. A green solution formed without any effervescence. Acetate of barytes scarcely rendered this solution turbid; but nitrate of silver produced a copious curd-like precipitate, and iron abundantly threw down copper from it. The green grains enclosed in this native sulphate of potash, appear, therefore, to be a submuriate of copper, of the same species as that of the green sands of Peru and Chili.

Muriatic acid dissolved the yellow ochraceous powder, and prussiate of soda-and-iron produced Prussian blue. I am inclined to believe this yellow powder to be a submuriate of iron, but its small quantity, and the admixture of the submuriate of copper, were impediments to entirely satisfactory results. Such a submuriate of iron, though, if I mistake not, overlooked by chemists, exists, for the precipitate which oxygen occasions in solution of green muriate of iron, contains marine acid.

Possibly this yellow powder, and the crystals of speculary iron which exist in this Vesuvian salt, have been produced by a natural sublimation of muriate of iron, similar to that of the experiment of the Duke d'AYEN, recorded by MACQUER,* and which was known long before to Mr. BOYLE and Dr. LEWIS.†

This Vesuvian salt, considered in its totality, has presented no less than nine distinct species of matters, and a more rigorous investigation, than I was willing to bestow on it, would probably add to their number.

July 3, 1813.

* *Dict. de Chemie, Art. Fer.*

† A course of practical chemistry by WILLIAM LEWIS, 1746, page 63, note *f*.

XXXI. *Some Experiments and Observations on the Substances produced in different chemical Processes on Fluor Spar. By Sir* Humphry Davy, *LL.D. F.R.S. V.P.R.I.*

Read July 8, 1813.

In the Bakerian Lecture, for 1808, I have given an account of an experiment on the combustion of potassium in silicated fluoric acid gas, in which the gas was absorbed, and a fawn coloured substance formed, which effervesced with water, and left, after its action on that fluid, a residuum which burnt when heated in oxygen, reproducing silicated fluoric acid gas; and I concluded from the phenomena, that the acid gas was decomposed in the process, that oxygen was probably separated from it by the potassium, and that the combustible substance was a compound of the siliceous and fluoric bases.

The experiment of burning potassium in silicated fluoric acid gas was made likewise by M. M. Gay Lussac and Thenard, before I published any account of my researches on this phenomenon. It was indeed one of the most obvious applications of potassium, and it occurred to many others, as well as to myself, that it might be made, immediately after I discovered that metal.

M. M. Gay Lussac and Thenard drew the same conclusions as I did, namely, that the acid gas was probably decomposed during the action of potassium on silicated fluoric acid;

but their general views differed from mine in this respect, as they supposed, that no part of the inflammable matter was derived from silica, and they likewise reasoned on the phenomena with more caution.

At the time that my conclusions were drawn, I was ignorant of the true nature of the muriatic acid. After I had tried in vain to decompose oxymuriatic gas, and after I had found that the compounds of this substance with phosphorus, sulphur, and the metals combined with ammonia without any decomposition, and produced compounds in which no oxygen could be discovered; I was forcibly struck by the analogy between the oxymuriatic and the fluoric compounds, and led to doubt of the justness of my ideas respecting the nature of fluoric acid.

I tried an experiment on the comparative quantities of fluate of lime, formed from equal volumes of silicated fluoric acid gas, one of which had been acted upon by potassium, and then exposed to solution of ammonia, the other had been absorbed by solution of ammonia: and I found the proportion of calcareous fluate nearly one-third larger in the latter case. This result at first seemed favourable to my early ideas, that the acid contained a peculiar inflammable basis, which was separated by the potassium, and existed in the combustible substance insoluble in water; but it could not be considered as decisive on the question, for, it occurred to me as possible, that this substance might be silicum, or the basis of silica united to a much smaller proportion of the fluoric principle, than that existing in silicated fluoric acid.

During the period that I was engaged in these investigations, I received two letters from M. Ampere, of Paris, con-

taining many ingenious and original arguments in favour of the analogy between the muriatic and fluoric compounds. M. AMPERE communicated his views to me in the most liberal manner; they were formed in consequence of my ideas on chlorine, and supported by reasonings drawn from the experiments of M. M. GAY LUSSAC and THENARD.

Before I enter upon the detail of the investigations which promise to elucidate the nature of the fluoric compounds, it will be right to describe those substances produced from fluor spar, which have been the principal objects of my experiments, and to mention the different hypothetical views that may be formed respecting them.

The first of these substances is the silicated fluoric acid gas, which was discovered by SCHEELE, and examined in its pure state by PRIESTLEY. It is formed by heating a mixture of fluor spar, powdered glass, and sulphuric acid. It is a very heavy elastic fluid, its specific gravity being nearly forty-eight times as great as that of hydrogen. It produces, according to my brother Mr. JOHN DAVY, a quantity of silica equal to $\frac{152}{1000}$ of its own weight by its action upon water, and a quantity equal to $\frac{614}{1000}$ of its weight by its action upon solution of ammonia. It condenses twice its own volume of ammonia, and forms a solid salt, volatile when free from water without decomposition.

Liquid fluoric acid, the second of these substances, was discovered by SCHEELE, but first obtained in its pure form by M. M. GAY LUSSAC and THENARD. It is procured by heating concentrated sulphuric acid and pure fluor spar, in retorts of silver or lead, and receiving the product in receivers of the same metals artificially cooled. It is a very active substance, and

must be examined with great caution. According to my experiments, its specific gravity is 1,0609.* It produces a high degree of heat when mixed with water, and such is its degree of attraction for water, that it becomes denser by combining with that fluid. By adding water, in very small quantities at a time, to pure liquid fluoric acid, I found that its specific gravity gradually increased till it became 1,25: it is, I believe, the only known body possessed of this property.

The third substance is fluo-boric acid gas, which was discovered by M. M. GAY LUSSAC and THENARD. It is produced by intensely heating, in an iron tube, a mixture of dry boracic acid and fluor spar, or by gently heating in a glass retort a similar mixture with sulphuric acid. Its specific gravity is rather more than thirty-two times as great as that of hydrogen. It forms a solid salt, volatile without decomposition, by condensing its own volume of ammonia. The ammoniacal salt dissolved in water and distilled, affords boracic acid.

The most important phenomena of chemical change, in which these bodies operate, that may be supposed to illustrate their nature, is their agency upon potassium and other metals. The action of potassium upon silicated fluoric gas has been already referred to. M. M. GAY LUSSAC and THENARD, by heating potassium and sodium in fluo-boric acid gas, obtained fluate of potassa or soda, and the basis of the boracic acid; and by exposing potassium to liquid fluoric acid, their results were hydrogen and acid fluate of potassa.

* Unless it is distilled through tubes and into vessels of pure silver, its specific gravity is greater; it readily dissolves tin, and slowly dissolves lead, and after being long kept in vessels of pure silver, it is found to have taken up a small portion even of that metal.

Three hypotheses may, according to sound analogies, be formed on the nature of the fluoric combinations. In the first, which is that generally adopted, the silicated fluoric acid gas is supposed to be a compound of silica and a peculiar acid, itself consisting of inflammable matter and oxygen; fluo-boric acid gas, a compound of boracic acid and the same acid; and pure liquid fluoric acid as water combined with the acid.

In the second hypothesis, that which I have alluded to in the beginning of this paper, and that adopted by M. AMPERE, the silicated fluoric acid is conceived to consist of a peculiar undecompounded principle, analogous to chlorine and oxygen, united to the basis of silica, or *silicum;* the fluo-boric acid of the same principle united to boron; and the pure liquid fluoric acid as this principle united to hydrogen.

In the third hypothesis, which probably would have been formed by the disciples of the phlogistic school of chemistry, had they been acquainted with the facts, the liquid fluoric acid is considered as an undecompounded body; and the metals and inflammable bodies as compounds of certain unknown bases with hydrogen: silicated fluoric acid gas, on this idea, must be regarded as a compound of the fluoric acid with the basis of silicum, and fluo-boric acid gas as a compound of fluoric acid and the basis of boron.

Whoever will consider, with attention, the different facts that have been brought forward by SCHEELE, GAY LUSSAC and THENARD, JOHN DAVY, and myself, will find that they will admit of explanation on either of these hypotheses; and, as in all the cases yet brought forward, of the most simple chemical action of other bodies on the fluoric substances, more than one new form of matter is produced, no explanation of

the phenomena can at present be given without involving suppositions.

It is not easy to devise simple experiments to ascertain which of these hypotheses is true, yet, in admitting strict analogical reasoning, it is easy to shew which is most conformable to the general series of chemical facts.

Those acids which are known by direct experiments of decomposition by heat, to consist of oxygen, bases, and water, such as the strongest sulphuric and nitric acids and hydrophosphorous acid, when they are acted on by ammonia, afford moisture: this is easily proved, by causing them to absorb ammoniacal gas in glass retorts, and gently heating the mixture, when water immediately appears. On this view, it occurred to me, if the liquid fluoric acid was a compound of water, and inflammable bases, and oxygen, that water ought to be produced when it was made to combine with ammonia. It was not possible to make the experiment in glass vessels, as the acid acts with great violence on glass, producing silicated fluoric acid gas. I had recourse, therefore, to an apparatus made of platina. A small tray of platina was filled with pure liquid fluoric acid, and introduced into a tube of platina connected by proper stop-cocks with a mercurial gazometer, filled with ammonia; the end of the platina tube was closed by a brass stopper, and a communication made between the ammonia and the fluoric acid; the ammonia was gradually absorbed, producing heat; and white fumes sometimes rose into the gas-holder, so that it was necessary from time to time to cut off the communication; ammoniacal gas was supplied till no more absorption took place. When the tube was quite cool, the stopper was removed, and the result examined; the

interior contained a white crystalline mass, but there was no appearance of fluid.* A polished brass tube, cooled by means of ice, was held over the aperture of the platina tube, and it was gently heated till the salt began to sublime, but no moisture was found condensed in the cold tube of brass.

This experiment is unfavourable to the idea, that the liquid fluoric acid contains water; and the following result is likewise unfavourable to the idea that it consists of an inflammable basis united to oxygen. Solid and perfectly dry fluate of ammonia was introduced into a tray of platina, with about an equal quantity of potassium, and the tray was heated in a small tube of glass connected with a mercurial apparatus. A violent action took place, gas was disengaged with great violence, which remained for some time clouded; the application of heat was continued till the tube was red: it was then suffered to cool, and the results examined. Much white matter, which proved to be fluate of potassa, had been carried by the violence of the action out of the tray of platina into the glass tube; and a little potassium had sublimed in the tube. The tray contained a considerable portion of potassium, and a saline matter, which had all the characters of fluate of potassa. The gas disengaged, consisted of ammonia and hydrogen, to each other in volume nearly as two to one; but the experiment cannot be considered as decisive on this point, as no particular precautions had been taken to dry the mercury.

* It is necessary that pure liquid fluoric acid, *i. e.* that which has the lowest specific gravity, be used for this experiment. The first time that I made it, I obtained moisture, owing to my having formed the hydro-fluoric acid by means of sulphuric acid that had not been previously boiled, and which must have contained more than one proportion of water.

Now, if there had existed oxygen combined with an inflammable basis in the fluate of ammonia, it might have been expected to have been separated, or at least to have formed a new combination during the action of potassium upon the fluate of ammonia, which is the case with such ammoniacal salts as contain acids in which oxygen is an element. Thus nitrate of ammonia acted on by potassium, as I have found, affords azote and ammonia; and sulphur is partly disengaged, and partly newly combined during the agency of potassium in excess upon sulphate of ammonia.

The action of potassium upon fluate of ammonia is precisely similar to its action upon muriate of ammonia, in which as I have found, by numerous experiments, ammonia and hydrogen to each other in volume as two to one are disengaged, and muriate of potassa (*potassane*) formed.

All the hydrates, that is, all the substances which contain definite proportions of water, united to acids, alkalies, or oxides, which are fluid, or capable of being rendered fluid by heat, when exposed to the chemical agency of Voltaic electricity, undergo decomposition, and their inflammable principles, either pure or combined with a smaller proportion of oxygen, are disengaged at the negative surface in the circuit, and their oxygen at the positive surface. Thus sulphuric acid affords sulphur and hydrogen at the negative surface, and the hydrophosphorous acid, phosphuretted hydrogen and phosphorus, and nitric acid nitrous gas; and all these bodies yield oxygen at the positive surface.

I undertook the experiment of electrizing pure liquid fluoric acid, with considerable interest, as it seemed to offer the most probable method of ascertaining its real nature; but consider-

able difficulties occurred in executing the process. The liquid fluoric acid immediately destroys glass, and all animal and vegetable substances; it acts on all bodies containing metallic oxides; and I know of no substances which are not rapidly dissolved or decomposed by it, except metals, charcoal, phosphorus, sulphur, and certain combinations of chlorine.

I attempted to make tubes of sulphur, of muriates of lead and of copper containing metallic wires, by which it might be electrized, but without success. I succeeded, however, in boring a piece of horn silver in such a manner, that I was able to cement a platina wire into it, by means of a spirit lamp, and by inverting this in a tray of platina filled with liquid fluoric acid, I contrived to submit the fluid to the agency of electricity in such a manner, that in successive experiments it was possible to collect any elastic fluid that might be produced. Operating in this way, with a very weak Voltaic power, and keeping the apparatus cool by a freezing mixture, I ascertained that the platina wire at the positive pole rapidly corroded, and became covered with a chocolate powder; gaseous matter separated at the negative pole, which I could never obtain in sufficient quantities to analyze with accuracy; but it inflamed like hydrogen. No other inflammable matter was produced when the acid was pure.

In a case in which the acid had been condensed in a tube of lead, joined by a solder containing tin, a large quantity of powder separated at the negative surface of a dark colour, and which appeared to be tin mixed with a subfluate; the powder burnt when heated in the air, and gave fluoric fumes when treated by potassa and sulphuric acid.

I attempted to electrize the liquid fluoric acid, by making plumbago the positive surface; but the plumbago was quickly destroyed, a subfluate of iron was deposited on the negative surface, and the liquid became turbid and black. When a point of charcoal attached to a wire of platina was made positive, the effects were similar to those produced by a platina wire alone, for the acid speedily penetrated through the pores of charcoal, and the platina, in consequence, became a point of contact with the fluid.

I applied the power of the great Voltaic batteries of the Royal Institution to the liquid fluoric acid, so as to take sparks in it. In this case, gas appeared to be produced from both the negative and the positive surfaces; but it was probably only the undecompounded acid rendered gaseous, which was evolved at the positive surface, for during the operation the fluid became very hot, and speedily diminished. The manner in which the surrounding atmosphere became filled with the fumes of the fluoric acid, rendered it, indeed, very difficult to examine the results of any of these experiments; the dangerous action of these fumes have been described by M. M. GAY LUSSAC and THENARD, and I suffered considerable inconvenience from their effects during this investigation. By mere exposure to them in their uncondensed state, my fingers became sore beneath the nails, and they produced a most painful sensation, which lasted for some hours, when they came in contact with the eyes.

The phenomena of the Voltaic electrization of fluoric acid, present no evidences in favour of its containing a peculiar combustible substance and oxygen; and the most simple mode of explaining them, is by supposing the fluoric acid, like

muriatic acid, composed of hydrogen, and a substance, as yet unknown, in a separate form, possessed, like oxygen and chlorine, of the negative electrical energy, and hence determined to the positive surface, and strongly attracted by metallic substances.

This view is much more conformable to the general order of chemical and electrical facts than the third hypothesis, just now mentioned.

It is indeed possible to conceive, if the metals be regarded as compounds of hydrogen, that the hydrogen may be produced from the metal, positively electrified at the time that the acid combines with its supposed basis, and that this hydrogen may be transferred to the negative surface; but this supposition involves a multitude of others; and the results of the electrization of fluoric acid are analogous to most of the results of the electrization of water and muriatic acid, both of which are shewn by analysis and synthesis to be compounds of hydrogen; and in the electrical decomposition of these bodies, their characteristic element is generally combined with the positive metallic surface.

In the Bakerian Lecture for 1810, I have given an account of the action of potassium upon pure silica. In this process, the potassium acquires oxygen, and a combustible substance, which consists either of the basis of silica, or the basis of silica combined with potassium appears. In supposing the silicated fluoric acid gas to be composed of this basis and the fluoric principle, it is easy to explain the action of potassium upon it, and the complicated phenomena, occasioned by the agency of water, and acids, and oxygen, on the results of this action. The potassium must be conceived to attract a part of the fluoric

principle from the siliceous basis, or to form a triple compound, from which silicated fluoric acid gas is capable of being reproduced, in consequence of the combination of a part of the potassium and siliceous basis with oxygen; and on this idea the cause of the apparent loss of the fluoric principle, in the experiments on the action of ammonia on the product of the combustion of potassium in silicated fluoric acid gas, becomes obvious.

Assuming then from the analogy with chlorine, that the different fluoric compounds consist of inflammable bodies united to a peculiar principle, it follows that all attempts to decompose the fluoric acids, by combustible substances, can lead to no other result, than that of occasioning new combinations of the fluoric principle; and the only methods which seemed plausible for obtaining this principle pure, after that by electrical decomposition had failed, were by the action of oxygen or chlorine on certain of its compounds. Chlorine is, in certain instances, detached from hydrogen by oxygen; and oxygen, in a number of cases, is detached from metals by chlorine; I thought it therefore probable, that the fluoric principle might, in some process, be separated from bases by either chlorine or oxygen.

In selecting compounds for experiments of this kind, I was guided by the relative attractions of the fluoric and muriatic acids, of chlorine and oxygen. Horn silver and calomel, and muriate of potassa are not decomposed by fluoric acid, but fluate of silver, of mercury, and of potassa are easily decomposed by muriatic acid; I therefore conceived, that the fluoric principle would most likely be expelled from the dry fluates of silver, mercury, and potassa by chlorine.

I made some pure fluates of silver and mercury, by dissolving the oxides of these metals in fluoric acid, and I heated them in small trays of platina; much fluoric acid was driven off in this process, which I continued in the case of the fluate of mercury till the salt began to sublime, and in that of the fluate of silver till it was red hot.

The dry salts were introduced in small quantities into glass retorts, which were exhausted and then filled with pure chlorine: the part of the retort in contact with the salt was heated gradually till it became red. There was soon a strong action, the fluate of mercury was rapidly converted into corrosive sublimate, and the fluate of silver more slowly became horn silver. In both experiments there was a violent action upon the whole of the interior of the retort. On examining the results, it was found that in both instances there had been a considerable absorption of chlorine, and a production of silicated fluoric acid gas, and oxygen gas.

I tried similar experiments, with similar results, upon dry fluates of potassa and soda. By the action of a red heat, they were slowly converted into muriates with the absorption of chlorine, and the production of oxygen, and silicated fluoric acid gas, the retort being corroded even to its neck.

The obvious explanation of these phenomena is, that a particular principle, the acidifying matter of the fluoric acid, combined with the metals, is expelled from them by the stronger attraction of the chlorine, and that this principle coming in contact with glass decomposes it by its attraction for the silicum and sodium, and separates them from the oxygen with which they were combined.

I made various attempts to procure the fluoric principle in a

pure form. I heated the fluates of potassa and soda in trays of platina, in a tube of platina connected with a vessel filled with chlorine. In this case the fluates were converted into muriates, with a considerable increase of the weight of the tray; and the platina was violently acted upon, and covered with a reddish brown powder; and in the instance in which fluate of potassa was used, a compound of fluate of platina and muriate of potassa was formed.

There was a considerable absorption of chlorine; but no new gaseous matter could be discovered in the gas in the tube.

I tried to obtain the fluoric principle pure, by decomposing the fluates in a tube of silver, but with no better success; the silver was acted upon both by the chlorine and the fluoric principle, and rapidly dissolved. I used glass tubes coated with resin of copper (*cuprane*) and hornsilver (*argentane*), on which I concluded that the fluoric principle would have no action from the decomposition of fluate of silver by chlorine; but at the degree of heat required to decompose the fluoric salts, the muriates were always fused, the glass violently acted upon, and silicated fluoric acid gas formed.

In one instance, in which fluate of potassa had been heated in a platina tray and tube, in which muriate of potassa had been fused, for the purpose of defending the interior, as much as possible, from the action of the fluoric principle, the gas, when disengaged into the atmosphere, had a peculiar smell, different from that of chlorine, (which certainly formed the greatest proportion of the elastic matter,) and more disagreeable; and dense white fumes were produced by its action upon the air. A portion of this gas thrown into a glass receiver, over

mercury, acted upon the glass, and silicated fluoric acid gas was generated. On examining the platina tray, however, it was found corroded, and the reddish brown powder formed.

In the course of these investigations, I made several attempts to detach hydrogen from the liquid fluoric acid, by the agency of oxygen and chlorine. It was not decomposed when passed through a platina tube heated red with chlorine, nor by being distilled from salts containing abundance of oxygen, or those containing abundance of chlorine.

I distilled the fluates of lead and mercury with phosphorus and sulphur, with the hope of obtaining compounds of the fluoric principle with phosphorus and sulphur. In all experiments of this kind, a decomposition took place, and the glass tubes employed were violently acted upon, and sulphurets and phosphurets were formed. When I used tubes lined with sulphur the decomposition was less perfect; but minute quantities of limpid fluid condensed in a part of the tube cooled by ice, both in the cases when sulphur and when phosphorus were used; it had the appearance of hydrofluoric acid, and speedily dissipated itself in white fumes. Whether they were that substance which had obtained its hydrogen from these inflammable bodies, or compounds of sulphur and phosphorus with the fluoric principle, I have not ascertained; but the first opinion seems most probable.

When I heated fluate of lead and finely powdered charcoal strongly in the air, the lead became revived, and white fumes were produced. I thought it probable, that in this case a compound of fluorine and charcoal was formed; but on trying the experiment in a close vessel of platina, no change took

place; and it evidently depended upon the presence of hydrogen in the vapour of the atmosphere, or in the flame of the spirit lamp, by which the experiment was made, and I found muriate of silver decomposed, and silver produced under the same circumstances.

From the general tenor of the results that I have stated, it appears reasonable to conclude that there exists in the fluoric compounds a peculiar substance, possessed of strong attractions for metallic bodies and hydrogen, and which combined with certain inflammable bodies forms peculiar acids, and which, in consequence of its strong affinities and high decomposing agencies, it will be very difficult to examine in a pure form, and, for the sake of avoiding circumlocution, it may be denominated *fluorine*, a name suggested to me by M. Ampere.

From experiments that I have made on the composition of the fluoric combinations, and which I shall soon have the honour of communicating to the Society, it appears that the number representing the definite proportion in which fluorine combines, is less than half the number representing that in which chlorine combines; and hydrates in becoming fluates lose weight, so that on the generally received idea of the existence of a peculiar acid in the fluates, and of their being compounds of oxides, with an acid containing oxygen, that acid, according to the law of definite proportions, must contain more oxygen in proportion to its quantity of inflammable matter than water, which is highly improbable, and contrary to all analogies.

Dr. Wollaston has found, that the fluoric combinations have very low powers of refracting light, and particularly the pure fluoric acid; so that the refracting powers of fluorine

will probably be found lower than those of any other substance, and it appears to possess higher acidifying and saturating powers than either oxygen or chlorine.

It is easy to perceive, in following the above theory, that all the ideas current in chemical authors respecting the fluoric combinations, must be changed. Fluor spar, and other analogous substances, for instance, must be regarded as binary compounds of metals and fluorine.

Many objects of enquiry arise, likewise, from these new views: the topaz contains the fluoric principle, but new experiments are required to shew whether that gem is a true silicated fluate of alumina, or a compound of the inflammable bases of alumina and silica with fluorine.

I have ascertained that the chryolite yields no silicated fluoric gas, when acted on by sulphuric acid, but merely pure fluoric acid; but I have not continued the research so far, as to determine whether it contains fluorine united to inflammable matter only, or fluorine and oxygen.

XXXII. *Catalogue of North Polar Distances of Eighty-four principal fixed Stars, deduced from Observations made with the Mural Circle at the Royal Observatory. By* John Pond, *Esq. Astronomer Royal, F. R. S.*

Read July 8, 1813.

THE Catalogue, which I have the honour to transmit to the Society, is deduced from the whole of the observations made with the mural circle, from its first erection in June, 1812, to the present time. I am still employed in endeavouring to give it a greater degree of precision, and when it is entirely finished, I propose to submit some of the observations themselves to the Society, and explain the method by which the results have been obtained.

I have already mentioned, that I use neither level nor plumb-line; but determine the position of the instrument by means of a standard catalogue of stars derived from the instrument itself, in such a manner, that every series of observation of these stars, serves the double purpose of ascertaining the position of the instrument, and at the same time improving the Catalogue.

As the present Catalogue has been formed by frequently changing the position of the telescope on the circle, for the purpose of correcting every possible error of division, my observations have not been calculated to decide the question of parallax which has been suspected to exist in α Lyræ, α

Aquilæ, and some other stars. But from this time forward, I propose to use the instrument in one position of the telescope, with the hope of ascertaining, if not the parallax of these stars, at least the limits, which it does not exceed.

Though the change of position in the telescope, by which all error of division is avoided, is one of the most beautiful properties of this instrument, yet so accurately is it divided, that I cannot perceive, with certainty, any effect produced by this change, for I have often found as great a discordance between two series of observations made on the same divisions, as when they are entirely changed by a new position of the telescope. What the error of division may amount to in any one position, I cannot exactly say; but, I think, when the six microscopes are used, it can never exceed half a second, and very rarely amounts to half that quantity.

That some opinion may be formed of the accuracy of this instrument, I have subjoined to the Catalogue the results of the observations of some of the standard stars, whose places I am anxious to determine with the greatest precision, since I propose in future to determine all north polar distances by comparison with these stars; precisely in the same manner as right ascensions are now determined by comparison with the thirty-six stars, whose places have been so accurately determined by Dr. MASKELYNE.

GENERAL CATALOGUE.

Names of Stars.	No. of Obs.	N. P. Distances Jan. 1, 1813.
		° ′ ″
1 γ PEGASI	25	75 51 21,0
2 α CASSIOP.	42	34 29 22,7
3 γ Cassiop.	8	30 17 53,0
4 POLARIS	200	1 41 21,75
5 δ Cassiop.	14	30 44 26,8
6 α ARIETIS	50	67 25 36,5
7 α CETI	18	86 39 0,75
8 α PERSEI	44	40 48 52,7
9 δ Persei	10	42 49 16,4
10 η Tauri	10	66 28 55,5
11 γ Eridani	9	104 2 51,2
12 γ Tauri	10	74 49 59,8
13 1 δ	9	72 54 18,3
14 2 δ	10	72 59 53,8
15 ε	10	71 14 39,2
16 ALDEBARAN	56	73 52 35,4
17 CAPELLA	80	44 12 20,5
18 RIGEL	30	98 25 33,8
19 β TAURI	50	61 33 43,6
20 γ Orionis	8	83 49 48,3
21 δ	18	90 26 48,6
22 ε	14	91 19 48,5
23 ζ	16	92 3 1,8
24 α ORIONIS	50	82 38 15,7
25 γ Geminorum	12	73 27 5,8
26 ε	11	64 41 41,4
27 SIRIUS	34	106 28 0,7
28 δ Geminorum	11	67 41 1,9
29 η Canis Major	6	118 56 42,7
30 CASTOR	30	57 42 46,7

Names of Stars.	No. of Obs.	N. P. Distances begin. 1813.
		° ′ ″
31 PROCYON	40	84 18 14,4
32 POLLUX	40	61 31 56,3
33 α HYDRÆ	10	97 51 11,3
34 ε Leonis	4	65 22 11,3
35 REGULUS	62	77 7 22,7
36 ζ Leonis	4	65 39 17,3
37 γ	4	69 12 59,7
38 α URS. MAJ.	60	27 14 31,5
39 δ Leonis	3	68 27 11,3
40 β LEONIS	24	74 22 57,7
41 γ URS. MAJ.	48	35 15 55,3
42 δ Urs. Maj.	13	31 55 38,2
43 κ Draconis	3	19 10 45,3
44 δ Virginis	3	85 34 58,6
45 α SPICA. VIRG.	20	100 10 51,3
46 η URS. MAJ.	80	39 44 57,9
47 α Draconis	12	24 43 38,2
48 ARCTURUS	80	69 50 19,1
49 θ Bootes	6	37 16 49,7
50 π	7	72 46 25,8
51 ε	12	62 7 52,4
52 1 } α LIBRÆ	15	105 15 22,7
53 2 } α LIBRÆ	4	105 12 38,7
54 β URS. MIN.	90	15 4 49,0
55 β Libra	8	98 41 3,4
56 α COR. BOR.	90	62 38 55,4
57 α SERPENTIS	70	82 38 39,3
58 δ Scorpii	9	112 4 41,4
59 1 β	8	109 16 55,7
60 2 β	7	109 16 43,6

Names of Stars.	No. of Obs.	N. P. Distances begin. 1813.
		° ′ ″
61 δ Ophiuchi	16	93 12 9,8
62 ANTARES	36	116 0 16,6
63 ζ Herculis	10	58 3 4,7
64 α HERCULIS	50	75 23 14,0
65 α OPHIUCHI	70	77 17 39,2
66 γ DRACONIS	90	38 29 3,7
67 α LYRÆ	100	51 23 0,5
68 ζ Aquilæ	13	76 24 19,0
69 δ Draconis	21	22 40 0,5
70 δ Aquilæ	10	87 14 53,4
71 γ Aquilæ	38	79 50 0,6
72 α AQUILÆ	100	81 36 58,7
73 β Aquilæ	12	84 3 4,1
74 1 α CAPRICORNI	35	103 5 35,4
75 2 α CAPRICORNI	28	103 6 52,3
76 α Delphini	10	74 44 23,9
77 α CYGNI	80	45 22 56,9
78 α CEPHEI	40	28 12 12,5
79 β Aquarii	13	96 23 11,9
80 β CEPHEI	50	20 5 30,6
81 δ Capricorni	12	106 58 6,6
82 α AQUARII	20	91 13 21,6
83 α PEGASI	20	75 47 51,8
84 α ANDROMEDÆ	35	61 56 29,6

RESULTS OF OBSERVATIONS.

β Ursæ Minoris.

1812, June 12, 13, 14, 16, 17, 21, 22, 23, 28, 29 -	49",659	Mean of 10	49",659
June 30, July 6, 8, 9, 18, 20, 21, 22, 28, Aug. 7 -	48,926	Mean of 20	49,292
Aug. 13, 15, 17, 18, 19, Sept. 15, 16, 20, 21, Oct. 2 -	48,996	Mean of 30	49,194
Oct. 4, 18, 13, 19, 21, 25, 26, 28, 29 - -	48,617	Mean of 40	49,049
Oct. 31, Nov. 9, 14, 18, 21, 22, 23, Dec. 7, 8, 12 -	48,485	Mean of 50	48,937
Dec. 14. 1813 May 21, 26, 27, 28, 29, 31, June 1, 4, 7	48,951	Mean of 60	48,939
June 8, 11, 12, 16, 21, 22, 25, July 4, 5, 6, - -	49,695	Mean of 70	49,047
July 9, 12, 16, 17, 18, 20, 22, 24, 25, 26 - -	48,651	Mean of 80	48,998
July 28, 29, 30, Aug. 4, 7, 9, 12, 13, 16, 18 -	48,616	Mean of 90	48,955

This result is probably exact to within a quarter of a second. The discordances seem quite accidental, they neither arise from parallax, nor error of division, for the three last series were made with the telescope in the same position, and consequently upon the same division, yet they differ more than observations usually do, which are made in different positions.

β Cephei.

1812, Oct. 22, 24, 26, 29, 31. Nov. 3, 4, 21, 22, Dec. 5	30″,574	Mean of 10	30″,574
Dec. 8, 9, 13. 1813, Mar. 16, 20, Apr. 1, 2, 3, 8, 14	30,343	Mean of 20	30,458
Aug. 11, 12, 13, 15, 16 17, 19, 20, 22, 23 - -	30,985	Mean of 30	30,634
Aug. 24, 25, 26, 30, 31, Sept. 2 3, 4, 5, 6 - -	30,894	Mean of 40	30,699

This result is probably exact to a quarter of a second.

α Ursæ Major.

1812, June 13, 29, July 9, 10, 15, 20, Aug. 14, Oct. 1, 2, 3	31″,500	Mean of 10	31″,500
Oct. 4, 6, 14, 15, 18, 19, 20, 23, 27, 28 - -	31,596	Mean of 20	31,548
Oct. 30, Nov. 4, 5, 9. 1813, Mar. 23, 26, 29, 31, Apr. 2, 3	31,525	Mean of 30	31,540
Apr. 4, 7, 8, 9, 10, 12, 13, 14, 20, 26 - - -	31,731	Mean of 40	31,588
May 24, 26, 31, June 1, 7, 8, 9, 11, 16, 25 -	31,124	Mean of 50	31,495
June 26, 27, July 6, 10, 18, 20, 25, 30, Aug. 5, 6 -	31,279	Mean of 60	31,459

This result seems to be extremely exact, and probably does not differ more than one-tenth of a second from the truth.

α Cephei.

1812, Oct. 28, 29, 31, Nov. 3, 21, 22, Dec. 5, 8, 9, 13	12″,484	Mean of 10	12″,484
1813, Mar. 20, Apr. 1, 3, 8, 12, Aug. 11, 12, 13, 15, 16	12,735	Mean of 20	12,610
Aug. 17, 19, 20, 21, 22, 23, 24, 25, 30, 31 - -	12,435	Mean of 30	12,551
Sept. 2, 3, 4, 5, 6, 7, 8, 9, 10, 14 - -	12,228	Mean of 40	12,470

Exact to a quarter of a second.

α Cassiop.

1812, June 17, 22, 28, July 6, 7, Oct. 26, 28, 29, 31, Nov. 3	22″,839	Mean of 10	22″,839
Nov. 4, 5, 6, 7, 19, 20, 22, 28, 29, Dec. 6 - -	22,456	Mean of 20	22,647
Dec. 7, 8, 9, 10. 1813, Jan. 9, 10, 16, 22, 24, Apr. 8 -	522,59	Mean of 30	22,630
Apr. 9, 11, 17, May 26, 27, 28, 31, June 7, 10, 12 -	23,041	Mean of 40	22,726

This result may, I think, be relied on, to a quarter of a second.

γ Ursæ Major.

1812, June 22, Oct. 27, 28, Nov. 3, 5, 6, 21, 24, Dec. 6. 1813, Mar. 17 -	55″,440	Mean of 10	55″,440
Mar. 21, Apr. 1, 3, 4, 7, 8, 9, 10, 11, 12 - -	54,975	Mean of 20	55,207
Apr. 13, 14, 16, 17, 20, 26, May 22, 26, 28, 29 -	55,489	Mean of 30	55,301
May 31, June 1, 7, 8, 12, 23, 25, 27, July 5, 6 -	55,457	Mean of 40	55,340
July 16, 20, 30, Aug. 7, 20, 31, Sept. 12, 14 - -	54,952	Mean of 48	55,275

This result, like the last, is probably exact to a quarter of a second.

γ Draconis.

1812, June 15, 16, 21, 23, 28, 29, July 6, 7, 8, 9 -	″ 3,845	Mean of 10	″ 3,845
July 10, 11, 14, 17, 19, 20, 21, 22, 28, 29 - -	3,779	Mean of 20	3,812
July 30, 31, Aug. 1, 3, 12, 13, 15, 17, 20, 21 - -	3,182	Mean of 30	3,602
Sept. 15, 16, 18, 19, 20, 21, Oct. 1, 3, 5, 8 - -	3,604	Mean of 40	3,603
Oct. 9, 15, 21, 24, 26, 28, 29, 31, Nov. 3, 6 - -	3,651	Mean of 50	3,612
Nov. 8, 15, 19, 20, 21, 22, 23, Dec. 5, 6, 8 - -	3,877	Mean of 60	3,656
Dec. 9, 10, 13, 15. 1813, June 22, 24, 25, 26, 27, 28 -	3,471	Mean of 70	3,635
July 5, 6, 9, 10, 11, 12, 13, 16, 17, 18 - -	3,980	Mean of 80	3,674
July 19, 20, 22, 23, 24, 25, 26, 27, 28, 29 - -	3,477	Mean of 90	3,652

This determination of γ Draconis is extremely exact. Its zenith distance has also been determined, with equal accuracy, by 120 observations made with the zenith sector during the years 1811 and 1812: the mean of above 60 observations of 1811 do not differ more than one-tenth of a second from the mean of an equal number in 1812.

	°	′	″
Mean zenith distance beginning of 1813 by observations of 1811 - - - -		2	17.8
By observations in 1812 - - - -		2	17.9
Mean zen. distance by zenith sector -		2	17.85
N. P. D. by mural circle - -	38	29	3.65
Sum or Co. latitude - - -	38	31	21.5

η Ursæ Major.

1812, June 11, 12, 13, 14, 15, 17, 20, 20, 22, 23, 24 -	57″,671	Mean of 10	57″,671
June 28, July 6, 7, 8, 9, 11, 15, 18, 20, 28 - -	58,069	Mean of 20	57,870
Aug. 7, 15, 19, 20, 22, Sept. 16, 18, 20, 21, Oct. 4 -	58,138	Mean of 30	57,966
Oct. 5, 7, 8, 24, 28, 30, Nov. 5, 6, 7, 14 - -	58,000	Mean of 40	57,969
Nov. 18, 21, 23, Dec. 5, 8, 23. 1813, May 26, 28, 31, June 1	57,348	Mean of 50	57,845
June 4, 7, 10, 11, 12, 16, 21, 22, 23, 25 - -	58,123	Mean of 60	57,891
June 26, 27, July 5, 6, 9, 16, 18, 19, 20, 22 -	58,057	Mean of 70	57,915
July 24, 27, 28, 29, 30, Aug. 3, 12, 13, 16, 19 -	57,641	Mean of 80	57,881

This result is probably not inferior in precision to the last.

α Persei.

1812, July 7, 9, 17, 20, 21, 28, Aug. 16, 17, Dec. 8, 12	52″,970	Mean of 10	52,970
Dec. 13, 14, 28. 1813, Jan. 8, 10, 11, 22, 24, 28, Feb. 5	52,417	Mean of 20	52,693
Feb. 23, March 1, 4, 6, 7, 17, 19, Apr. 11, 12, 13 -	52,468	Mean of 30	52,618
Apr. 14, 18, May 27, 28, June 1, 19, 20, July 6, 15, 17	52,824	Mean of 40	52,670

Exact to a quarter of a second.

Capella.

1812, June 15, 26, 28, July 7, 8, 9, 10, 14, 20, 23 -	20,987″	Mean of 10	20,987″
July 28, 30, Aug. 13, 16, 17, 19, 21. 1813, Jan. 2, 7, 8	21,009	Mean of 20	20,998
Jan. 10, 11, 16, 22, 24, 27, 28, 31, Feb. 23, Mar. 1 -	20,719	Mean of 30	20,905
Mar. 2, 3, 5, 6, 7, 12, 17, 18, 22, Apr. 8 - -	19,951	Mean of 40	20,667
Apr. 9, 10, 11, 12, 13, 14, 15, 17, 18, 20 - -	21,124	Mean of 50	20,758
Apr. 21, May 28, 29, June 1, 2, 20, July 4, 5, 6, 12 -	19,537	Mean of 60	20,555
July 15, 16, 17, 22, 23, 25, 28, 29, 30, 31 - -	20,258	Mean of 70	20,512
Aug. 3, 5, 6, 8, 9, 10, 11, 13, 15, 17 - -	20,061	Mean of 80	20,468

One of the above series differs a second from the mean of the whole, a circumstance very unusual, but quite unconnected with error of division, I attribute it to want of sufficient care in reading off the microscopes. The mean result may, nevertheless, be depended upon to a quarter of a second.

α Cygni.

1812, Sept. 15, 16, 17, 18, 20, 21, Oct. 2, 3, 4, 5 -	56″,846	Mean of 10	56″,846
Oct. 8, 10, 13, 14, 16, 22, 23, 24, Nov. 7, 8 - -	56,568	Mean of 20	56,707
Nov. 15, 19, 20, 21, 22, 23, 24, Dec. 8, 9, 10 - -	56,980	Mean of 30	56,798
Dec. 13, 30. 1813, Jan. 8, 9, 10, 16, 22, Feb. 8, 9, 10	57,405	Mean of 40	56,925
Feb. 15, 28, March 6, 11, 12, 16, 20, 26, 27, Apr. 1 -	57,178	Mean of 50	56,975
July 27, 28, 29, 30, Aug. 3, 4, 5, 7, 9, 10 - -	56,665	Mean of 60	56,940
Aug. 11, 12, 13, 15, 17, 19, 20, 21, 22, 23 - -	56,887	Mean of 70	56,947

Result very exact.

α Lyræ.

1812, July 6, 7, 9, 10, 14, 17, 21, 22, 23, 26 -	0″,123	Mean of 10	0″,123
July 28, 30, 31, Aug. 1, 6, 12, 13, 15, 16, 17 - -	0,514	Mean of 20	0,318
Aug. 18, 19, 20, 21, Sept. 15, 16, 18, 19, Oct. 1, 2 -	0,364	Mean of 30	0,334
Oct. 3, 4, 5, 8, 9, 10, 12, 15, 16, 19 - -	0,299	Mean of 40	0,325
Oct. 21, 24, 26, 28, 29, 31, Nov. 2, 3, 7, 8 -	0,983	Mean of 50	0,457
Nov. 13, 15, 19, 20, 21, 22, 23, Dec. 2, 6, 8 - -	0,857	Mean of 60	0,522
Dec. 9, 10, 12, 13. 1813, Jan. 2, 7, 9, 15, 21, June 22	0,514	Mean of 70	0,521
June 23, 24, 25, 26, 27, 28, July 5, 6, 9, 10 - -	0,501	Mean of 80	0,519
July 11, 12, 13, 16, 17, 18, 19, 23, 24, 25 - -	0,187	Mean of 90	0,482

Castor.

1812, Aug. 13, 19, 21. 1813, Jan. 16, 24, 28, Mar. 5, 7, 10, 16 - -	46″,916	Mean of 10	46″,916
Mar. 18, 23, 31, Apr. 3, 8, 11, 13, 15, 28, June 1 -	46,672	Mean of 20	46,794
Aug. 11, 14, 16, 23, 24, 28, Sept. 2, 6, 7, 10 -	46,596	Mean of 30	46,728

Pollux.

1812, June 13, 15, 19, Aug. 13, 16, 17, 19, Oct. 1, 3, 4	56″,536	Mean of 10	56″,536
Oct. 11. 1813, Jan. 16, 22, 24, Feb. 28, Mar. 5, 10, 13, 18, 23	56,332	Mean of 20	56,434
Mar. 31, Apr. 8, 11, 13, 15, May 28, June 1, 25, July 28, 30	56,313	Mean of 30	56,394
Aug. 3, 5, 11, 16, 22, 23, 24, 25, 28, Sept. 1 -	56,380	Mean of 40	56,341

Exact to a quarter of a second.

β Tauri.

1812, July 20, 22, 30, Aug. 13, 19, 21, Nov. 19, 28. 1813, Jan. 2, 8 - -	43″,650	Mean of 10	43″,650
Jan. 10, 11, 13, 24, 25, 27, 28, 31, Mar. 1, 3 -	43,438	Mean of 20	43,544
Mar. 5, 6, 7, 12, 17, 18, 19, 31, Apr. 9, 10 - -	43,410	Mean of 30	43,499
Apr. 11, 12, 14, July 15, 16, 17, 25, 28, 29, 30 - -	43,903	Mean of 40	43,600
July 31, Aug. 6, 8, 9, 11, 15, 18, 20, 22, 23 -	43,820	Mean of 50	43,663

Exact to less than a quarter of a second.

α Cor. Bor.

1812, June 11, 12, 13, 14, 15, 19, 21, 22, 23, 28 -	″ 55,441	Mean of 10	″ 55,441
June 30, July 4, 6, 8, 9, 15, 18, 19, 20, 21 - -	55,295	Mean of 20	55,368
July 22, 26, 28, Aug. 13, 15, 17, 18, 19, Sept. 15, 16	55,316	Mean of 30	55,351
Sept. 19, 20, 21, Oct. 3, 4, 5, 6, 8, 13, 20 - -	56,041	Mean of 40	55,523
Oct. 26, 28, 29, 31, Nov. 20, 21, 22, Dec. 7, 12, 14 -	55,791	Mean of 50	55,577
1813, Jan. 8, 9, 11, May 29, June 7, 8, 10, 12, 16, 18	54,833	Mean of 60	55,453
June 21, 22, 24, 25, 27, 29, July 5, 6, 10, 12 -	55,008	Mean of 70	55,389
July 16, 17, 18, 19, 20, 23, 24, 26, 27, 28 - -	55,487	Mean of 80	55,401

Exact to less than a quarter of a second.

α Arietis.

1812, June 18, 19, 22, 23, 26, 28, July 6, 7, 9, 10 -	″ 37,380	Mean of 10	″ 37,380
July 14, 17, 20, 21, 22, Aug. 17, Oct. 24, 25, 26, 28 -	36,263	Mean of 20	36,822
Oct. 29, 31, Nov. 3, 5, 6, 7, 8, 9, 15, 19 - -	35,811	Mean of 30	36,485
Nov. 21, 22, 24, 28, 29, Dec. 6, 7, 8, 13. 1813, Jan. 8	36,532	Mean of 40	36,497
Jan. 9, 16, May 22, June 19, 20, July 4, 5, 6, 11, 16	36,474	Mean of 50	36,492

I mentioned this star as doubtful in my former Catalogue. I believe the discordances were quite accidental, and that the mean result is within a quarter of a second of the truth.

Arcturus.

1812, June 11, 12, 13, 15, 16, 19, 20, 21, 28, 29 -	18,729	Mean of 10	18,729
July 4, 6, 7, 8, 9, 11, 15, 18, 20, 26 - -	19,867	Mean of 20	19,298
July 18, 31, Aug. 7, 14, 18, 19, Sept. 20, 21, Oct. 2, 4 -	19,341	Mean of 30	19,312
Oct. 7, 9, 13, 16, 18, 24, 25, 26, Nov. 3, 5 - -	19,225	Mean of 40	19,291
Nov. 6, 7, 14, 18, 21, 22, 23, Dec. 6, 7, 8 - -	19,453	Mean of 50	19,323
Dec. 12, 14, 23. 1813, May 24, 26, 27, 28, 31, June 1, 4	18,761	Mean of 60	19,230
June 7, 8, 10, 11, 12, 16, 21, 22, 25, 26 - -	18,585	Mean of 70	19,137
June 27, July 5, 6, 7, 9, 12, 16, 18, 19, 23 - -	18,653	Mean of 80	19,077

Exact to a quarter of a second, or less.

Aldebaran.

1812, June 23, 28, July 5, 7, 8, 9, 10, 25, 26, 28 -	35,614	Mean of 10	35,614
Aug. 14, 16, 17, 19, 21, Sept. 5, Dec. 25, 28. 1813, Jan. 8, 10	35,135	Mean of 20	35,374
Jan. 11, 16, 22, 24, 28, 31, Feb. 23, Mar. 1, 6, 7 -	35,475	Mean of 30	35,407
Mar. 11, 17, 18, 19, 31, Apr. 4, 8, 9, 10, 11 - -	35,390	Mean of 40	35,402
Apr. 12, 14, 15, June 28, July 6, 8, 10, 15, 16, 17 -	35,347	Mean of 50	35,392
July 23, 25, 27, 28, 29, 30	35,142	Mean of 56	35,365

Result very exact.

β Leonis.

1812, June 13, 14, 15, 22, 29, July 6, 8, 9, 11, 12 -	57″,544	Mean of 10	57″,544
July 15, 23, Oct. 2, 3, 4, 6, 7, 14, 15, 22 - -	57,077	Mean of 20	57,310

This star has not been often observed, owing to its proximity in right ascension to γ Urs. Maj.; but the result is probably exact to within 0″,3, as it very seldom happens that the mean of twenty observations differs a quarter of a second from the truth

α Herculis.

1812, July 11, 14, 30, Aug. 3, 7, 15, 17, 18, 20, Sept. 19	13″,835	Mean of 10	1″,3835
Sept. 20, 21, Oct. 3, 4, 8, 9, 29, 31, Nov. 15, 21 -	14,170	Mean of 20	14,002
Nov. 22, 23, Dec. 12. 1813, Jan. 9, 11, 15, July 11, 25, 27, 28 - -	13,803	Mean of 30	13,936
July 29, 30, Aug. 2, 4, 7, 9, 10, 12, 13, 15 - -	14,223	Mean of 40	14,008
Aug. 17, 18, 19, 23, 24, 30, 31, Sept. 5, 6, 13 -	14,169	Mean of 50	14,040

Result very exact.

α Pegasi.

1812, Oct. 1, 2, 3, 4, 5, 8, 10, 11, 12, 16 - -	51″,330	Mean of 10	51″,330
Oct. 17, 21, 28, Nov. 3, 7, 9, 19, Dec. 5, 7, 12 -	52,060	Mean of 20	51,695

This star is reserved for future examination.

Regulus.

1812, June 12, 13, 15, 19, 20, 22, 23, July 6, 7, 8 -	23,331	Mean of 10	23,331
July 9, 14, 15, 18, 21, 23, 28, Oct. 1, 2, 3 - -	23,205	Mean of 20	23,268
Oct. 4, 6, 12, 14, 15, 19, 20, 23, 27, 28 - -	22,543	Mean of 30	23,026
Nov. 5. 1813, Mar. 7, 10, 12, 13, 17, 18, 22, 23, 26 -	22,236	Mean of 40	22,829
Mar. 29, 31, Apr. 1, 2, 3, 4, 7, 8, 9, 11 - -	22,148	Mean of 50	22,692
Apr. 12, 13, 14, 20, 26, May 24, 28, 31, June 2, 7 -	22,665	Mean of 60	22,688
June 8, 11. July 6, 7, 9 -	22,576	Mean of 65	22,679

Not to be relied on with the same confidence as some others, as one of the above series differs a second from the mean; the result may be erroneous a quarter of a second, but, I think, not much more.

α Ophiuchi.

1812, June 28, 29, July 11, 14, 21, 22, 26, 28, 30, Aug. 3	39″,126	Mean of 10	39″,126
Aug. 19, 20, Sept. 15, 16, 19, 20, 21, Oct. 3, 8, 9 -	39,949	Mean of 20	39,537
Oct. 28, 31, Nov. 7, 15, 19, 21, 22, 23, Dec. 6, 8 -	40,358	Mean of 30	39,811
Dec. 29. 1813, Jan. 7, 9, 15, June 24, 25, 26, 27, July 5, 6	39,215	Mean of 40	39,662
July 9, 10, 11, 12, 13, 16, 17, 18, 19, 22 - -	38,849	Mean of 50	39,500
July 23, 24, 25, 26, 27, 28, 29, Aug. 2, 4, 5 - -	38,523	Mean of 60	39,339
Aug. 7, 9, 10, 12, 13, 15, 17, 19, 20, 24 - -	38,115	Mean of 70	39,163

Some discordances in the above observations, which I cannot explain, render the result doubtful, and I reserve this star for future examination.

α Aquilæ.

1812, July 26, 30, Aug. 1, 3, 6, 9, 12, 14, 18, 19 -	58,133	Mean of 10	58,133
Sept. 15, 16, 18, 19, 20, 21, Oct. 2, 3, 4, 7 - -	59,381	Mean of 20	58,757
Oct. 8, 10, 13, 15, 16, 19, 21, 23, 24, 27 - -	58,963	Mean of 30	58,826
Oct. 28, 29, Nov. 2, 3, 6, 7, 15, 19, 21, 22 - -	59,705	Mean of 40	59,045
Nov. 23, 24, Dec. 8, 9, 10. 1813, Jan. 21, Feb. 28, Mar. 5, 6, 7 - -	59,626	Mean of 50	59,162
Mar. 12, 20, 26, 27, Apr. 1, 3, July 11, 12, 16, 17 -	58,014	Mean of 60	58,970
July 19, 21, 22, 25, 26, 27, 28, 29, 30, Aug. 1 -	58,280	Mean of 70	58,872
Aug. 2, 3, 4, 7, 10, 11, 12, 13, 15, 16 - -	58,280	Mean of 80	58,799

Procyon.

1812, June 12, 19, July 25, Sept. 15, 16, 17, 18, 19, 20. 1813, Feb. 24 - -	13,999	Mean of 10	13,999
Mar. 3, 6, 8, 17, 22, 29, Apr. 2, 4, 9, 10 - -	14,221	Mean of 20	14,110
Apr. 12, 14, 20, June 2, 7, 8, July 29, Aug. 11, 14, 16	14,795	Mean of 30	14,338
Aug. 17, 23, 24, Sept. 2, 6, 7, 10, 12, 13, 14 -	14,426	Mean of 40	14,360

Probably exact to a quarter of a second.

α Orionis.

1812, July 17, 22, 25, Aug. 5. 13, 14, 16, 17, 19. 1813, Jan. 2 - -	16,044″	Mean of 10	16,044″
Jan. 10, 11, 13, 16, 22, 25, 27, 28, Mar. 2, 11 -	15,486	Mean of 20	15,765
Mar. 18, 22, Apr. 3, 4, 9, 10, 11, 12, 13, 14 -	16,380	Mean of 30	15,970
Apr. 17, 18, 19, 20, May 28, July 8, 16, 17, 28, 29 -	15,849	Mean of 40	15,940
Aug. 6, 8, 11, 13, 18, 19, 20, 23, 24, Sept. 1 -	14,856	Mean of 50	15,723

From the discordance of the last series, I consider this result as doubtful.

α Serpentis.

1812, June 12, 13, 17, 19, 22, 23, 28, 30, July 2, 4 -	38,187″	Mean of 10	38,187″
July 6, 8, 9, 15, 18, 20, 21, 22, 26, 28 - -	39,508	Mean of 20	38,847
Aug. 14, 15, 17, 18, 19. Sept. 15, 16, 18, 21, Oct. 3 -	39,700	Mean of 30	39,132
Oct. 4, 13, 26, Dec. 6, 7, 12. 1813. Jan. 8, 9, 15, June 7	39,282	Mean of 40	39,169
June 8, 10, 13, 16, 18, 21, 22, 24, 25, 27 - -	39,244	Mean of 50	39,184
July 5, 10, 11, 12, 16, 17, 18, 19, 20, 22 - -	39,670	Mean of 60	39,265
July 23, 26, 27, 28, 29, 30, Aug. 2, 7, 12, 15 -	39,240	Mean of 70	39,260

Result very exact.

Polaris.

Mean Day of the Month.	Position of the Telescope on the Instrument.	Number of Observations above the Pole.	Number of Observations below the Pole	Total Number of Observations.	N. P. D. Jan. 1, 1813.		
1812.					° ′ ″		″
June 15	0	3	8	11	1 41 21,79	Mean of 11	21,79
23	30	4	3	7	22,06	Mean of 18	21,89
July 7	10	4	4	8	22,49	Mean of 26	22,08
18	20	6	5	11	22,29	Mean of 37	22,14
Oct. 18	0	10	9	19	21,69	Mean of 56	21,99
Nov. 10	10	10	6	16	21,71	Mean of 72	21,95
Dec. 8	20	10	7	17	21,32	Mean of 89	21,83
1813.							
Apr. 1	30	19	17	36	21,44	Mean of 125	21,72
June 10	0	19	23	42	21,70	Mean of 167	21,72

The mean of more than 200 observations of this star is 1° 41′ 21″,75. The above 167 were selected in preference; the result, however, is the same.

Notwithstanding the great number of observations of this star, there are discordances which render the result doubtful to 0″,25. If the observations with four microscopes, previous to Oct. 1812, be rejected, the mean result with six microscopes will be 1° 41′ 21″,6, which I prefer to the above.

The following Table shews the State of the Standard Catalogue at this present Time, Sept. 1813.*

N. P. D. of Stars for the beginning of the Year 1813.

	Names of Stars.	No. of Obs.	N. P. D. Jan. 1813.			Difference of former Catalogue.	
			°	′	″		
1	Polaris	200	1	41	21,75	+ 0,02	
2	β Urs. min.	90	15	4	48,95	0,00	
3	β Cephei	40	20	5	30,70	+ 0,40	
4	α Urs. maj.	60	27	14	31,46	— 0,04	
5	α Cephei	40	28	12	12,47	+ 0,12	
6	α Cassiop.	40	34	29	22,73	— 0.10	
7	γ Urs. maj.	50	35	15	55,27	+ 0,17	
8	γ Draconis	90	38	29	3,65	— 0,08	
9	η Urs. maj.	80	39	44	57,83	+ 0,08	
10	α Persei	40	40	48	52,67	+ 0,30	
11	Capella	80	44	12	20,47	— 0,49	
12	α Cygni	800	45	22	56,92	= 0,27	
13	α Lyræ	100	51	23	0,43	— 0,29	
14	Castor	30	57	42	46,73	+ 0,16	
15	Pollux	40	61	31	56,34	— 0,23	
16	β Tauri	50	61	34	43,66	+ 0,15	
17	α Androm.	35	61	56	29,61		The same as in former Catalogue,
18	α Cor. Bor.	90	62	38	55,43	— 0,27	and probably true to 0″,5 or less.
19	α Arietis	50	67	25	36,49	— 0,27	
20	Arcturus	80	69	50	19,08	+ 0,04	
21	Aldebaran	56	73	52	35,36	+ 0,18	
22	β Leonis	20	74	22	57,31	+ 0,07	
23	α Herculis	50	75	23	14,04	+ 0,07	
24	α Pegasi	20	75	47	51,70	+ 0,07	
25	Regulus	50	77	7	22,69	— 0,25	
26	α Ophiuchi	70	77	17	39.16	— 0,50	Doubtful to 0″,5.
27	α Aquilæ	100	81	36	58,66	— 0,22	Doubtful.
28	α Orionis	30	82	38	15,72	— 0,24	Doubtful to 0″,5.
29	α Serpentis	70	82	58	39,26	+ 0,39	
30‡	Procyon	40	84	18	14,36	— 0,36	

‡ The N. P. D. of Procyon in the former Catalogue was 15″,03; this was from a mistake of 1″,0 committed in adding the annual variation, it should have been 14″,03.

* Though the Observations were given to the Society, as by the date of the paper, yet, by the permission of the President and Council, they were extended till the time that they went to the press.

Remarks on the above Observations.

α Lyræ and α Aquilæ having been supposed subject to a sensible parallax, I have, as I mentioned before, reserved them for future examination. The observations which I have already made on these stars, and particularly on α Aquilæ, are not incompatible with this supposition, though I cannot at present venture to decide whether the small discordances I have met with are to be attributed to any regular cause, or are only accidental.

Whenever I speak of the degree of exactness to which any result may be depended upon, I allude only to the mechanical measure given by the instrument. I have every reason to believe, that if two fixed and well defined points could be placed in the plane of the meridian, I could, in a very short time, measure their angular distance to within a tenth of a second; but astronomers must be well aware that the stars are not presented to us in this simple form, and that the sources from which small errors may arise, either in the observations themselves or subsequent computation, are so very numerous, that anomalies will occur even to the most careful observer, which he will in vain endeavour to explain. With respect to the parallax of α Lyræ, I might observe that it is a star so badly defined, and so little adapted for exact observation, that a parallax of half a second would not be easy to determine even with the Greenwich circle.

α Aquilæ is in some respects a better star for observation, but only half its actual parallax would be sensible in declination.

There are several other stars much better adapted for this investigation, even should their distance be supposed more than double, such are Polaris, η Ursæ maj. α Cygni, β Urs. min. and γ Draconis; now in these I have not hitherto found any sensible parallax; occasional discordance has frequently suggested some slight hopes, but these have always been disappointed by continuing the observations. It is, however, useless now to anticipate this subject farther.

Those stars which are in the general Catalogue, but which do not form part of the standard Catalogue, I presume to be exact to the nearest second.

I have not included any star in the standard Catalogue south of the equator, on account of the uncertainty of refraction.

XXX Observa of the Sun er Solstice 1813, with the Mural Circle, at the Royal Obs ato.

By Joh Pond, Esq. Astronomer Royal, F. R. S.

Read uly 8, 1813.

1813.	Barometer.	Therm. In.	Therm. Out.	Refraction.	Observations as given by the Instrument.	Equations for N. P. D.	Equations for Zenith Distance.	Semidiameter of the ⊙ by Nautical Almanack.	Reduction to the Solstice.	Solstitial Zenith Distance with Parallax.	Solstitial N. P. D. with Parallax.	Correction for ⊙'s Lat.	Solstitial Zenith Distance corrected for ⊙'s Lat.	Solstitial N. P. D. corrected for ⊙'s Lat.
				′ ″	° ′ ″	″	° ′ ″	′ ″	′ ″	° ′ ″	° ′ ″	″	° ′ ″	° ′ ″
June 10	29,57	62	69	30,3	⊙ LL 67 14 21,9	— 0,6	— 38 31 22,1	15 46,5	— 26 44,7	28 0 58,9	66 32 20,4	+ 1,0	28 0 59,9	66 32 21,4
11	29,81	64	71	29,6	⊙ UL 66 38 18,9	— 0,6	22,1	15 46,5	22 16,2	56,7	18,2	+ 1,0	0 57,7	19,2
12	29,70	64	74	30,1	⊙ LL 67 5 48,1	— 0,6	22,1	15 46,4	18 12,1	57,6	19,1	+ 1,0	0 58,6	20,1
13	30,02	61	66	30,9	⊙ UL 66 30 36,9	— 0,6	22,1	15 46,3	14 32,4	59,6	21,1	+ 0,9	1 0,5	22,0
15	29,62	60	66	30,1	⊙ LL 66 56 2,3	— 0,6	22,1	15 46,1	8 26,7	57,5	19,0	+ 0,7	0 58,2	19,7
21	30,15	57	60	30,0	⊙ UL 66 16 9,3	— 0,6	22,1	15 45,8	0 1,9	1 1,1	22,6	— 0,1	1 1,0	22,5
23	30.17	56	59	30,8	⊙ LL 66 48 7,4	— 0,6	22,1	15 45,6	0 32,0	0 58,5	20,0	— 0,3	0 58,2	19,7
25	30,18	59	64	29,9	⊙ UL 66 18 47,8	— 0,6	22,1	15 45,6	2 41,1	1 0,1	21,6	— 0,3	0 59,8	21,3
27	30,07	64	70	30,2	⊙ LL 66 54 6,5	— 0,6	22,1	15 45,6	6 29,3	0 59,7	21,2	— 0,2	0 59,5	21,0
28	29,94	61	67	29,7	⊙ UL 66 25 9,0	— 0,6	22,1	15 45,6	9 0,4	1 1,8	23,3	— 0,1	1 1,7	23,2
29	29,75	64	74	30,0	⊙ LL 66 59 32,2	— 0,6	22,1	15 45,5	11 56,1	0 58,5	20,0	0,0	0 58,5	20,0
July 1	29,67	62	65	29,6	⊙ UL 66 35 7,1	— 0,6	22,1	15 45,5	19 0,7	0 59,4	20,9	+ 0,3	0 59,7	21,2
2	29,75	60	62	31,8	⊙ LL 67 10 44,8	— 0,6	22,1	15 45,5	23 9,5	59,5	21,0	+ 0,4	0 59,9	21,5
							Mean of 13 Observations		-	28 0 59,1	66 32 20,6		28 0 59,5	66 32 21,0
							Parallax — 4″,0	Nutation	— 6″,5 =	— 10,5	— 10,5		— 10,5	— 10,5
										28 0 48,6	66 32 10,1		28 0 49,0	66 32 10,5
										Mean Obliquity	-	-		23 27 49,5
										* Mean Obliquity at Summer Solstice, 1812				23 27 50,5
							Mean of Two Observations or Mean Obliquity, Jan. 1, 1813				-	-		23 27 50,0

* I avail myself of this opportunity of correcting a very small error that was made in computing the summer solstice of 1812. The correction for the sun's latitude should have been 0″,6 instead of 0″,9, and should have been applied with the contrary sign. The obliquity thus corrected will be 23° 27′ 50″,5.

PRESENTS

RECEIVED BY THE

ROYAL SOCIETY,

From November 1812 *to July* 1813,

WITH THE

NAMES OF THE DONORS.

1812.	PRESENTS.	DONORS.
Nov. 5.	A Description of the Collection of ancient Marbles in the British Museum with Engravings. Part I. London, 1812. 4°	The Trustees of the British Museum.
	A Treatise on Theatres, by George Saunders. London, 1790. 4°	Mr. George Saunders.
	Tracts on Mathematical and Philosophical Subjects, by Charles Hutton, LL.D. and F.R.S. London, 1812. 3 Vols. 8°	Dr. Charles Hutton.
	The Elements of Botany, or Outlines of the Natural History of Vegetables, by Benjamin Smith Barton, M.D. Philadelphia, 1812. 8°	Dr. Benj. Smith Barton.
	Elements of Chemical Philosophy, by Sir Humphry Davy. Part I. Vol. I. London, 1812. 8°	Sir Humphry Davy.
	Essai sur la Rage, par M. J. F. A. Lalouette. 8°	Mr. J. F. A. Lalouette.
	Institutions de Physique, par B. G. Sage. Paris, 1811. 3 Vols. 8°	Mr. B. G. Sage.
	Of the Circle and the infinite Incommensurability of its Area to the Square of its Diameter, with a Demonstration of Dr. Matthew Stewart's 42d Proposition, by James Glennie, Esq. F. R. S. 4°	James Glennie, Esq.
	The Speech of His Royal Highness the Duke of Sussex in the House of Lords, April 21, 1812. 4°	The Editor.
	An Account of a Case of Recovery, after an extraordinary Accident, in which the Shaft of a Chaise had been forced through the Thorax, by William Maiden. London, 1812. 4°	Mr. William Maiden.
	Eloge Historique de M. Sabatier, par M. Percy. Paris, 1812. 8°	Mr. Percy.
	Della Cometa del 1811, osservata nella Specola di Palermo, par Gius. Piazzi. 4°	Mr. Piazzi.

1813.	PRESENTS.	DONORS.
Nov. 5.	Observations on Dr. Sturges Pamphlet respecting Non-residence of the Clergy, in a Letter from T. B. Howell, Esq. to Mr. Baron Maseres. London, 1803. 8°	T. B. Howell, Esq.
	The Philosophical Magazine from June to October, 1812. 8°	Mr. A. Tilloch.
	The Critical Review from June to October, 1812. 8°	The Proprietor.
	A Journal of Natural Philosophy, Chemistry, and the Arts. No. 148 to 152. 8°	Mr. W. Nicholson.
	The European Magazine from January to October, 1812.	The Editor.
12.	A Catalogue of the Archiepiscopal Manuscripts in the Library at Lambeth Palace, with an Account of the Archiepiscopal Registers and other Records there preserved. fol.	The Archbishop of Canterbury.
	Illustrations of the Lotus of Antiquity, by R. Duppa, LL.B. London, 1813. 4°	R. Duppa, Esq.
	Kongl Vetenskaps Academiens nya Handlingar, Tom. XXX, XXXI, XXXII, for the Years 1809, 1810, and 1811. 8°	The Royal Academy of Sciences of Stockholm.
	A Sermon preached before the incorporated Society for the Propagation of the Gospel in Foreign Parts, Feb. 1, 1812, by Samuel Lord Bishop of Carlisle. London, 1812. 4°	The Bishop of Carlisle.
	De l'Opération de la Cataracte, par A. E. Tartra. Paris, 1812. 4°	Mr. A. E. Tartra.
	Quelques Idées nouvelles sur le Système de l'Univers, par Guil. Ant. Marechal. Paris, 1810. 8°	Mr. G. A. Marechal.
	Mon Opinion sur la Formation des Aerolithes, par Guil. Ant. Marechal. Paris, 1812. 8°	———————
19.	Abstract of Answers and Returns made pursuant to an Act (51 George III.) for taking an Account of the Population of Great Britain, and of the Increase and Diminution thereof, 1811. fol.	The Speaker of the House of Commons.
	Comparaison des Organes de la Mastication des Orthopterès, par M. Marcel de Serres. 4°	Mr. Marcel de Serres.
	De l'Odorat et des Organes qui paroissent en être le Siége chez les Orthoptères, par M. Marcel de Serres. 4°	———————
	Supplement aux Institutions de Physique, par B. G. Sage. Paris, 1812. 8°	Mr. B. G. Sage.
	Flora Lapponica, Auctore Geor. Wahlenberg. Berlin, 1812. 8°	Mr. G. Wahlenberg.
	A Journal of Natural Philosophy, Chemistry, and the Arts. No. 153.	Mr. W. Nicholson.
Dec. 10.	A Journal of Natural Philosophy, Chemistry, and the Arts. No. 154.	———————
	The Philosophical Magazine for November.	Mr. A. Tilloch.
	The Critical Review for November.	The Proprietor.
	The European Magazine for November.	The Editor.

1812.	PRESENTS.	DONORS.
Dec. 17.	Sui Mezzi immediate di estirpare la Povertà in Sicilia, per D. Giuseppe Emmanuale Ortolani. Palermo, 1812. 4°	Mr. Jos. Emmanuel Ortolani.
24.	Reports from the Commissioners of Public Records, with an Account of their Proceedings from 1800 to 1812. fol.	The Commissioners of Public Records.
	Rotuli Hundredorum Temp Hen. III. et Edw I. In Turr. Lond. et in Curia Receptæ Scaccarii Westm. asservati. Vol. I. fol.	————
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	Medico-Chirurgical Transactions, published by the Medical and Chirurgical Society of London. Vol. III. London, 1812. 8°	The Medical and Chirurgical Society of London.
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CORRIGENDA.

Page 175, line 13, *for* one-fourth—*read* one-third
177, — 17, *for* preponderates over that of either the olive oil or phosphorus—*read* be exceeded by that of the alcohol of sulphur.
246, — 10, *after* attraction of—*add* the azote of
——— 23, *for* produced—*read* produced,—and dele the comma after portion
—— 249, — 11, *for* 261.—*read* 268.
264, — 18, *for* the other—*read* the other of which
—— 268, — 15, *for* bases,—*read* basis
—— 273, — 11, *for* metal, positively electrified—*read* metal positively electrified,

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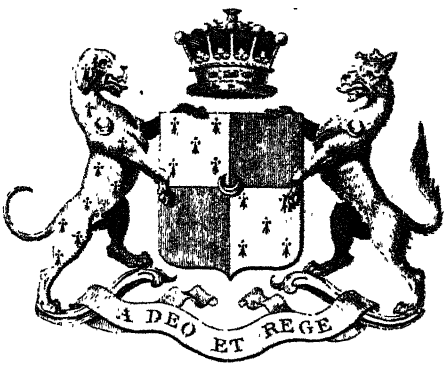
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